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ORIGINAL ARTICLES.

THE BACILLUS TUBERCULOSIS AND THE ÆTIOLOGY OF TUBERCULOSIS. IS CONSUMPTION CONTAGIOUS?

SECOND COMMUNICATION.

[Read before the Philadelphia County Medical Society, Nov. 14, 1883.]

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GENERAL CONSIDERATION.

A little over a year ago[1] I had the honor of presenting for your consideration some anatomical points in refutation of the ætiological relations of the bacillus tuberculosis.

At that time I announced some original observations regarding the histology of scrofulous tissue, tending to place the question of heredity in tuberculous disease upon an anatomical basis. These peculiarities of scrofulous tissues, I submitted as elucidating the ætiology of tuberculosis, showing that the peculiar histological condition of the individual, under the influence of simple irritants, and not the character of the irritant, is responsible for tubercular inflammation. It gives me pleasure to state that these observations have been confirmed by several competent histologists, whose articles on this subject will soon appear in print; besides which, a general interest has been manifested by favorable comments both in America and abroad.

Shortly before the publication of these observations, Koch, of Berlin, had brought forward the discovery of the now famous bacillus tuberculosis, affirming it to be the sole cause of pulmonary phthisis and other forms of tubercular disease, and claiming for it, besides exclusive pathogenetic properties, special morphological and chemical characteristics.

In my first paper, I denied some of these propositions upon grounds of personal investigation, and, subsequently, Koch's researches were also severely criticised by a number of other observers.

As interesting and valuable as the discovery of Koch is, from a biological standpoint, its practical value is, in my opinion, decidedly over-estimated, and has not nearly the significance for medical science which the enthusiastic followers of Koch ascribe to it. The influence of the discovery was, however, great in strengthening the traditional and unwarranted belief in the contagiousness of phthisis, as held by a small part of the profession and community. On the other hand, this belief led to the popularity of the discovery. In this respect the bacillus theory has perhaps been harmful, and, taking the consequences into consideration, we should not accept such a theory without the closest scrutiny.

Two practical benefits may accrue from this discovery. The first is, that the fear of the effects of the bacillus may induce greater cleanliness in hospital management, and enforce improvement in hygienic matters in general. It is doubtful whether the removal and prompt destruction of the sputum would have any influence in checking the spread of phthisis, as the disease is found as often, if not oftener, in the clean palaces of the wealthy, as in the unclean huts of the poor. The second benefit resulting from the bacillus theory may be, that physicians may become induced to make more use of the microscope in diagnosis; yet, in this respect, the general use of the microscope is hardly practicable, on account of the thorough technic and experience required.

To-day, while the bacillus is acknowledged as a common morphological concomitant of tubercle, the pathogenetic properties are denied it by the best pathologists and clinicians, on account of a want of sufficient confirmation of the evidence thus far offered.

The followers of Koch's theory are, however, numerous, but they are recruited largely from the ranks of clinical teachers, book-writers, and others possessing no opportunities for personal investigation.

It may be well to state that, upon my visit to Koch last summer, made with the purpose of doing justice to this important question, I was gratified in many respects. I found Koch an earnest and conscientious worker, and not as dogmatic and extreme in his views as would appear from his writings; nor is he as self-satisfied and as rash to jump at conclusions as are some of his followers. Koch has the coöperation of an excellent staff of assistants, all able mycologists; but it was a matter of surprise to me that there was not a single competent pathologist connected with Koch's laboratory; and such services are evidently

[1] Studies from the Pathological Laboratory of the University of Penn., XI.; "The Bacillus Tuberculosis and some Anatomical Points which suggest the Refutation of its Ætiological Relation with Tuberculosis, by H. F. Formad." Read before this Society, October 18, 1882.

much needed, to give to the observations made there the proper interpretation from a biological and anatomical standpoint. I was also pleased to learn in Berlin that the discovery of the bacillus was exaggerated, not so much by Koch himself as by the Imperial Board of Health, which employs him, and by his over-zealous followers in the profession. There is strong evidence, however, that Koch's investigations are biased by the determination to find for each specific disease a specific fungus.

Following out the various phases in the study of tuberculosis, I am sorry to see that the entire subject is now being considered from a purely ætiological basis with reference to bacteria, while the study of the anatomical and biological relations is wholly neglected.

I admire the beautiful bacteridian discoveries of Klebs, and particularly those of Koch in connection with the ætiology of tuberculosis. The accomplishment of these results is a triumph for mycology and scientific botany; but these studies are much too one-sided to have an application to scientific medicine. The bacillus is there! It is concomitant with most tubercular lesions. It is diagnostic of tuberculous change. It is, on account of its irritant properties, one of the causes of tuberculosis. But this forms no reason for asserting that tuberculosis should be considered a contagious disease, without further investigation and proof. A contagious disease can have only one cause. I cannot agree with those who define the predisposition to phthisis as being a condition of the organism which offers a favorable soil for the tubercle bacillus. Nor can I believe that inheritance is explained by subsequent infection from cohabitation; *e. g.*, that children become scrofulous by living with consumptive parents.

The latest fruits of the bacillus studies have even inspired Baumgarten (Centralblatt f. d. Med. Wis., Aug. 4, 1883) and several others to come to the conclusion, in reference to the hereditary nature of tuberculosis, that the bacillus is transmitted in its larval state from mother to fœtus in intra-uterine life! One would think, however, that one of the most wonderful effects of the tubercle bacillus is manifested by the change it produced in the direction of the reasoning of some of our pathological and clinical investigators, both at home and abroad.

Some of the younger pathologists are affected by a regular fanaticism for bacterian studies in tuberculosis. These studies now take the place of their former excellent pathologic-anatomical studies. Consideration is no longer given to the tissue changes, or the nidus which invites the bacteria and nourishes them. In fact, Koch's followers in their enthusiasm exaggerate matters, and, to Koch's own amusement, go further in their bacillus speculations than he himself thinks justifiable. It is really painful to read how some of the younger German pathologists, and a few of the prominent English surgeons, under the influence of the bacillus craze, will make, in their publications, assertions entirely unwarrantable. They describe, for instance, with the greatest ingenuity and exquisite minuteness, how "one or more bacilli" will produce certain histological changes in the lungs or in the peritonæum, designating the exact route to the same; how the different cells, the lymphatic and the blood-vessels are affected; how the bacilli convert one variety of cells into another; how they manufacture giant cells and cheesy material; how acute and chronic phthisis are produced by the bacilli, and the quantity necessary for each; how tubercles develop only and exactly in those places where the bacillus becomes lodged; how, if bacilli alone are inhaled, miliary tubercles form; and how, if the bacillus is accompanied by some other irritants, a broncho-pneumonia will ensue.

All the above statements are made by scientific medical men and pathologists, and offered as broad facts, in full earnest! I only have to say that here evidently observation is substituted by imagination and speculation; and all this is done for the sake of the convenience in explaining a disease by pretty hypotheses.

The only men who attempted to repeat Koch's experiments, besides the work done in the pathological laboratory of the University of Pennsylvania, were Spina (Studien über Tuberculose, Wien., 1883), and Watson Cheyne. Of the latter two scientists, Spina came to results entirely different from those of Koch, and they disprove beyond doubt some parts of Koch's hypothesis. From an analytical and critical point of view, Spina's studies of tuberculosis are excellent, but the technical part of his investigation is deficient, and hence not satisfactory. Watson Cheyne, to whom the "British Association for Advancement of Science by Research" had entrusted the investigation of tuberculosis, and the testing of Koch's researches, did not do justice to his mission. From Cheyne's report (*The Practitioner*, April, 1883), it is seen that he made no earnest attempt to study the nature of tuberculosis, because all he did was to study and experiment with bacteria met with in tuberculous lesions. He went to see some of the different mycologists, consulting only believers in the germ theory; obtained some French and German bacteridian material, and, after testing the same, he reports with great emphasis that Koch's bacilli are a more genuine tubercular virus than Klebs' or Toussaint's micrococci. He did not inquire, nor did he care, whether tuberculosis may have any other cause! He simply imitated some of Koch's experiments with the bacillus material in rabbits and guinea-pigs (only), and obtained, of course, the same results. Furthermore, he made some control experiments, which, as I will show, pass for nought, as they are much more deficient than those of Koch.

There are a number of excellent studies in reference to the occurrence of bacilli in the sputum and in tuberculous tissues; but the main part of Koch's hypothesis, *i, e.*, the ætiological relation of these bacilli to tubercular disease, remains still unconfirmed.

My own researches on tuberculosis were made from a standpoint different from that of Koch, and they were undertaken five years ago, being carried on continuously since that time by myself and assistants. My object was to investigate the natural history of the disease, without being influenced by any preconceived views. While due attention was paid to ex-

ternal agencies in the production of tuberculosis, the part played by the animal or human organism itself, the behavior of its component cells, and the primary changes in the tissues, were not lost sight of.

I may state that I was fortunate enough to be able to utilize the material of over four hundred cases of tubercular disease from the autopsy tables, including a number of cases studied in the pathological institutes in Europe at various times.

My present research on tuberculosis with special reference to the bacillus question, was carried on during the last year and a half, under the auspices of the Provost of the University of Pennsylvania, Dr. Wm. Pepper. This communication should not be considered a report on my investigations, as these are not yet concluded; but a detailed report of these investigations will be made next summer. Some of the positive results achieved will, however, be referred to in the present paper; otherwise it merely embodies a general critical survey of the question of the ætiology of tuberculosis, based upon a careful perusal of the literature of the subject and upon personal observation.

I may state at the outset that, while the results of my observations force me to make some concessions to Koch, namely, that his bacillus, on account of its irritative properties, can produce tuberculosis under certain conditions, I am firmer than ever in my former conclusions, from the results of repeated observations, that tuberculosis may arise from other causes. The bacillus may be one of the causes, conditionally, but it is not *the* cause. The question of predisposition stands in the way of the acceptance of the bacillus theory. Furthermore, I will try to show that tuberculosis is not a contagious disease, and it is particularly in reference to this that I am glad to bring the present subject before the Society, desiring to profit by the discussion which is to follow as a result of the experience and the clinical observation of the individual members of the Society.

The question of the contagiousness of phthisis is one of supreme importance, not only from its scientific, but also from its social aspects.

For convenience in treating the subject of the ætiology of tuberculosis, I shall speak of it under the following headings:

1. The definition, the anatomical character, and the ætiology of tubercular lesions, including pulmonary phthisis.
2. The predisposition; the predisposing conditions; scrofulosis.
3. Tuberculosis without predisposition, due to inflammation of serous membranes.
4. Question of contagiousness; clinical aspect.
5. The bacillus tuberculosis.
6. Experiments—"pro" and "contra;" traumatic tuberculosis. Conclusions.

All these considerations will have to be, of necessity, very brief.

I.—THE DEFINITION, THE ANATOMICAL CHARACTER, AND THE ÆTIOLOGY OF TUBERCULAR LESIONS, INCLUDING PULMONARY PHTHISIS.

No definite understanding concerning a disease can be arrived at unless some fixed conception of the anatomical characters and various expressions of the lesions of that disease is formed. Thus, as regards the question of tuberculosis and pulmonary phthisis, the matter would be much simpler if a general understanding could be arrived at as to the definition of tuberculosis and phthisis in its different anatomical manifestations. The pivot of the question is, what to call a tubercle or a tubercular lesion.

The traditional conception of a tubercle being a miliary node, the belief is that nothing is tuberculosis unless expressed by nodes, and that everything is tuberculosis that appears to the eye as containing nodes. These misconceptions are what bring the confusion and prevent the settlement of the question of tuberculosis, both at the post-mortem table and in the hands of the experimenter.

One of the results of this confusion is that some clinicians divide pulmonary phthisis into catarrhal, cheesy, fibroid and tubercular proper, because they do not see tubercle nodules in some of these forms of phthisis. They seem not to be aware of the fact that miliary tubercles do not belong necessarily to the picture of pulmonary phthisis; and, on the other hand, that those nodes which occasionally appear as miliary tubercles are not miliary tubercles at all; but are only miliary foci of broncho-pneumonia, due to aspiration, as will be explained later. Miliary tubercles, if at all present, usually form a part of a general disease, a tuberculosis of the whole body. In rare instances, when the miliary eruption takes its departure from the lung, the miliary nodules may be limited to the lung.

A more serious matter is the mistake that experimenters make of interpreting as tubercles the so-called inhalation tuberculosis, artificially produced in animals by means of a spray with tuberculous and other matter. The nodules produced in the lung under these circumstances are not miliary tubercles—in fact, no tubercles at all. They are simply miliary broncho-pneumonic foci, limited to those terminal collections of air-vesicles, called acini, in which some of the inhaled irritative material became lodged. The natural round boundaries of these acini correspond exactly to the usual size of miliary tubercles, and appear as such even under the microscope, although filled merely with an unorganized inflammatory exudate. The uniform distribution of these foci is due to the fact that the inhaled irritating particles are distributed only to individual and the most accessible bronchioles and acini, thus simulating a true miliary tuberculosis of the lung. Similar broncho-pneumonic foci occur in the human lung from self-aspiration of tuberculous material from a primary focus to some other portion of the lung, or throughout the whole lung. This was proven long ago, but the inhalation experimenters appear not to be aware of that fact. Careful personal observations and experiments, to be recorded in my forthcoming report, have convinced me that such inhalation experiments prove nothing, either for or against the contagiousness of tuberculosis, in connection with which they have been brought forward as the strongest affirmative proofs. Furthermore, it must also be remembered

that the so-called experimental inhalation tubercles, as a rule, remain local.

On the other hand, miliary nodes or tubercles are met with, not only in tubercular lesions, but also in a variety of similar and dissimilar lesions, such as pearl disease or bovine tuberculosis, lupus, leprosy, glanders, actinomycosis, chancre and gummata, cancer, typhoid infiltration, lymphomatous and leukæmic lesions. All these lesions, even cancer ("miliary carcinosis"), are able to give rise to exquisite miliary disseminations, or eruptions, although these are most frequently observed in tuberculosis. We already recognize leprous, lupous, glanderous, syphilitic and other tubercles, in contradistinction to tuberculous or scrofulous tubercles.

To the above nodular formations may be added a variety of minute inflammatory foci of granulation tissue, organized around minute foreign bodies introduced experimentally into various tissues; also, "false tubercles," such as mere unorganized collections of lymphoid cells, held together by some fibrine or by some artificial or natural round boundaries, such as is the case with the referred to "inhalation tubercles;" and further, also, the eruptions and follicular enlargements in the skin and mucous membranes.

The question now arises, how to distinguish between these various kinds of nodules, apart from their clinical features. They may all undergo a cheesy or a fibrous change, may calcify, and may contain giant cells. In all, bacilli may be found if a cheesy change occurs, or tends to occur, save in cancer and in leukæmic formation. Without desiring to appear skeptical, I must say, however, that it takes the skill of a Koch to differentiate sometimes the bacilli met with in the various kinds of nodes, even after applying all micro-chemical tests.

The true tuberculous tubercles occasionally do not show any bacilli whatever, as I will prove from personal observation, and from the reliable testimony of others. It will also be shown that the only test now left for determining the pathogenic peculiarity of tubercle—namely, the asserted exclusive property to produce tuberculosis—is conditional and uncertain, since substances, not tuberculous, may, under similar conditions, have the same effect.

Therefore, it is impossible to define tuberculosis, either by its anatomical peculiarity or by the pathogenic property of its nodes.

Another important point in the natural history of tuberculosis is the cheesy degeneration of its products; but here, again, we are surrounded by difficulty, if we take only the cheesy product into consideration, because all the lesions mentioned before as being characterized by, and as being capable of, nodular eruptions, have the tendency to undergo cheesy change. Besides this, simple inflammatory products have been observed to undergo a similar change, as is instanced by that form of cheesy hepatization sometimes following croupous pneumonia, and also by certain forms of rapid necrotic changes, such as occur in acute septic inflammations, designated lately by the name of coagulation necrosis. It must, however, be remembered that the total absence of cheesy masses in the body of tuberculous subjects has been observed.

To tell tuberculosis from allied lesions is only possible after a consideration of the soil in which it develops, and the location of the products, together with the clinical and anatomical manifestations.

What is the origin of tubercle nodules?

The primary occurrence of miliary tubercle nodes is, to my mind, very questionable. I have never seen it occur without the co-existence of diffuse granulation tubercle. This granulation tubercle is recognized by all as being a simple inflammatory granulation tissue, characterized by cells somewhat larger than ordinary lymphoid cells, containing usually giant cells, but undergoing very readily cheesy change on account of its deficiency in blood-vessels. This tissue is regarded by most pathologists as secondary to miliary tubercles; but I think, after careful observation, that the reverse is the case; because I have never seen upon the post-mortem table, or in animals, *primary* miliary nodes without the granulation tissue, while the granulation tubercle tissue does exist very frequently without the nodes. Moreover, primary *miliary* tuberculosis is unknown.

That tubercle is primarily a simple granulation tissue of inflammatory origin has been proven experimentally. E. Ziegler (Centralbl. f. d. Med. Wis., 1874, No. li) made the following interesting experiment: He inserted below the skin or into the peritonæum of animals, a number of pairs of glass covers, each pair glued together in such a manner that between them there existed an interspace just large enough to allow the entrance of white blood corpuscles; and these corpuscles, not being severed from the body of the animal, then formed a tissue between these plates of glass, which, upon removal after various periods, could be readily examined under the microscope, and the conditions of tissue formations traced. Under these circumstances it was observed that whenever blood-vessels had developed in the new-formed tissue between the glass plates, an organization of the cells into a perfect connective tissue took place; but, when the formation of blood-vessels had failed to occur, then a tissue simulating tubercle tissue was formed, made up of epitheloid and giant cells, and cheesy changes had occurred. Ziegler very properly declared the latter product to be tubercle tissue. I have had, and have at present, ample opportunity to corroborate the accuracy of these observations. Ziegler's experiments were repeated in the pathological laboratory of the University of Pennsylvania by Hammer, and at present are being carried on by Woodnut. By these experiments, made with but slight modification, after the method of Ziegler, under varying conditions and upon various animals, it was shown that the granulation tissue gradually gave origin to tubercle nodules. Furthermore, these experiments showed that the tubercle nodes and cheesy changes ensue without the action of bacilli, as the latter were found not to be present when proper care was taken, during the execution of the experiment, to exclude them.

From the examination of tubercular tissue from various sources, I may say that I have seldom suc-

ceeded in finding tubercle bacilli in newly-formed tubercular tissue made up of small lymphoid cells. In older tubercular tissue, made up of opaque epithelioid cells and giant cells with a nodular arrangement, particularly when this tissue is undergoing necrotic change, bacilli are quite common, except in some forms of tubercles of serous membranes, to be referred to later. Tubercle tissue that has undergone a complete cheesy change, contains the greatest number of bacilli. Cheesy matter of any source is a dead substance, and it is usually inhabited by bacilli, if these get access to it; while other bacteria are scarce on this nidus.

Examination of materials from the autopsy table shows that tubercle expresses itself in various manners. Primarily, tubercle occurs as a mere infiltration of lymphoid cells in the adventitia of blood-vessels, or as small nodular masses of lymphoid infiltration around blood vessels or ducts of any kind; or tubercle tissue may organize within blood-vessels and various ducts. Sometimes tubercle appears as a diffuse lymphoid infiltration, extending over a larger area, showing a greater or less tendency to the formation of nodes and cheesy or fibroid change, as in the lungs. Tubercle tissue may form masses of the size of a hen's egg, particularly in the brain and serous membranes. In the lungs, in racemose glands, and in mucous membranes, catarrhal changes always follow the tubercle infiltration. On serous surfaces primary tubercles appear often as flat or nodulated patches of various sizes (in peritonæum), or as fungoid vegetations (in synovial cavities), or even as large plastic masses (in omentum). In the skin and mucous membranes, tubercles produce eruptions, ulcers, or nodular indurations; in bones—caries, with abscess formation in surrounding parts (cold abscesses). Fibroid capsules, made up of connective tissue, due to reactive inflammation, enclose often smaller or larger tubercular masses, especially if these have undergone cheesy change.

Primary tubercle manifests itself quite variously in different animals. In guinea-pigs and rabbits, it appears mainly as small cellular infiltrates; in dogs, it often undergoes a fibroid change; in goats, and especially in cattle, tubercle often forms large nodular, sometimes pedunculated masses which often calcify;[1] in birds, it forms, preferably in the liver, large round mulberry masses, which, on section, appear sometimes as horny, radiating structures.

Secondary tubercle presents an aspect entirely different from primary tubercle, and it manifests itself in nearly all instances in but one form, namely, as a fine miliary eruption representing those well-known gray semi-transparent nodules of the size of a millet seed, called miliary tubercles. These seem to lie in the perivascular lymph-spaces, and are probably distributed throughout the body mainly by means of these lymph-channels of the blood-vessel walls. Tubercles do not occur in avascular tissues. There is, however, a second form of embolic or metastatic tuberculosis, which evidently distributes itself by the blood-current proper, and it appears in the form of conical masses or round nodes, which may reach the size of a walnut, and are located usually at the bifurcation of arteries. No mention of this form of tubercle is made in text-books, although upon the post-mortem table this variety of tubercle is a very common occurrence. Especially is it seen in the lung, and more rarely in the spleen and liver.

Taking into consideration the enormous frequency of local tubercular lesions (counting pulmonary phthisis into this category), the occurrence of secondary or true miliary tuberculosis must be considered a rare affection. A tuberculosis affecting the lining of even the whole peritoneal cavity, including its lymphatic glands, or that of the pleural sacs, or that involving one or both lungs, must, when occurring thus in but one locality, be considered a local tuberculosis. In such instances, the tubercle spreads by continuity of structure.

It is a fact, established by Virchow, that tuberculosis is at first a local disease, and only become generalized secondarily. This generalization does not affect the blood like in infectious diseases, but it takes place simply as an embolic process, like in some tumors. Local tuberculosis in external organs and accessible lymph-glands is often a harmless affection. It is strongly related to primary tumors. Complete early removal of local tubercular lesions is practiced successfully in Europe. Volkmann and others have removed, for instance, lymphatic glands, testes, and joints affected with fungoid synovitis, with the object of preventing secondary tuberculosis, and have thus prevented a general miliary tuberculosis.

Nor should a gloomy prognosis be given in early phthisis. It is astonishing what a large number of healed cavities and cicatrices in the apices of lungs are found on the post-mortem table, indicating the healing of phthisis in persons who long subsequently died from some other causes in later life.

We have seen that tuberculosis manifests itself quite differently as to structure, appearance, distribution and termination in the various animals, and even differently in the various organs of one individual. Our studies have shown that these variations in the expression of tuberculosis depend upon the structural peculiarities of the various kinds of animals, and sometimes even upon the difference of the structure in animals of the same species. We have also seen that even in human beings tubercle tissue may manifest itself in various forms. In some individuals it develops rapidly, and spreads over large areas, becoming generalized and undergoing speedy cheesy change; in other individuals it develops slowly, fibroid change predominating; and in others the tuberculous product may calcify. In most individuals tubercular lesions may remain entirely local.

It is well known from clinical experience that the general condition of the organism has very much to do with the healing of a local tuberculosis. A local tuberculous inflammation may heal or become arrested in its progress, if the patient "gets strong," or it becomes more developed and aggravated if his

[1] I have met with, on the autopsy table of the Philadelphia Hospital, two cases of tuberculosis in man that were identical in every respect to bovine tuberculosis. Dr. Creighton, of Cambridge, England, describes a number of cases from his own observation, and collected from literature. Bovine Tuberculosis, London, 1881.

general health "runs down." Observation has further shown that any simple non-specific wound in a weak, ill-nourished individual may fail to heal, becoming unamenable to treatment, and probably assuming a tubercular character.

In some animals spontaneous tuberculosis is unknown, and while some animals are easily tuberculizable experimentally, in others tuberculosis can not be produced.

It is in accordance with experience that in a large number of families the predisposition to tuberculosis is hereditary, and that their members die promptly of phthisis at a certain age from the effects of a simple "cold," while in the history of other families this affection is unknown. Every individual is liable to acquire syphilis, small-pox, and other contagious diseases, but it is proven that not every one can have tuberculosis. A special predisposition and a special individual are required. In such an individual, a simple inflammation resulting from any cause whatever, can produce tuberculosis.

Therefore, for the development of tuberculosis, two conditions are necessary:

1. A *definite* soil.
2. An *indefinite* irritant.

The reaction of the soil is always the same under the influence of any irritant, whether that irritant be a bacillus or not; since the result (tuberculosis) following a lesion in such a soil depends upon the character of the soil, and not upon the character of the irritant, even though one irritant, say bacilli, may act more readily than other irritants.

In view of the demonstrated fact that simple injuries of any kind can excite a tuberculosis, but only in certain individuals and tissues, it is evident that tuberculization is determined by the kind of soil and, not by a specific irritant. *Tubercle should, therefore, be defined as being an inflammatory new formation in a special individual or tissue.*

What is the place for tubercle in pathology? The anatomical criterion for tubercle is a granulation tissue made up of lymphoid or epithelioid cells, which, on account of deficiencies in the soil, does not undergo any higher organization, nor tend to heal; but tends to form nodes and undergo cheesy change. Under favorable circumstances it may heal through fibroid change. The elements of tubercle tissue may spread by continuity of structure to surrounding parts, and occasionally tend to the production of metastasis, distributing themselves by means of the lymphatic system principally, and rarely by blood-vessels; and may generalize themselves through the whole body, forming miliary nodes or tubercles.

This miliary eruption of tubercle appears to have the same relation to the primary tubercular growth as the secondary metastatic cancer eruption has to the primary cancerous growth. Like in cancer, the elements of tuberculosis may be arrested temporarily by the lymphatic glands governing the affected region.

In tuberculosis, lymphoid cells form the nodes; in cancer, epithelial cells. While secondary cancer nodes are, as a rule, much larger than tubercle nodules on account of the well-known great proliferating power of epithelium, it is also a fact that cancer may appear as a miliary carcinosis, expressed by minute nodules, not distinguishable macroscopically from miliary tuberculosis. Cancer is proven to be a local disease. It is not contagious. It is infectious only to the individual who is affected by it; *i. e.*, it is self-infectious. And so is tubercle, in every respect, a local, self-infectious disease.[1]

That local manifestation of tuberculosis in the lung, which is designated by the traditional name of pulmonary phthisis, forms perhaps nine-tenths of all tubercular lesions, and hence deserves some special consideration.

I arrange myself with those who regard all forms of pulmonary phthisis as tubercular. There are only three or four lesions of chronic wasting disease of the lung which may be excluded from phthisis. These are atelectasis, or collapse from pressure of effusions; bronchiectasis, in which the enormous dilatation of the bronchi may lead to large cavities and atrophy of lung structure; primary fibroid changes; and abscess of lung. Yet all these lesions may become tuberculous from secondary inflammatory changes which usually follow.

The lesions that are known as catarrhal pneumonia, bronchio-pneumonia, pneumonic phthisis, cheesy pneumonia, tubercular phthisis, and fibroid phthisis, are all manifestations of the one disease. Such a classification may be, however, entirely justifiable and useful for practical clinical and therapeutic purposes. Pathologically considered, phthisis is a local tuberculous inflammation of the lung, which may manifest itself in various ways, the appearance depending upon the duration of the disease, the mode of onset, and the constitution and condition of the patient. Lesions representing the different forms of phthisis, and their transition from one form to the other, are often seen in the same lung.

Virchow insists that nothing should be considered tubercular unless it shows true tubercle nodules, and hence he does not recognize cheesy pneumonia—or cheesy hepatization, as he calls it—as tubercular, although he does not object to the term phthisis for this lesion.

I was fortunate enough to attend several times the demonstrations on this point of Virchow, the father of the view of the dual origin of cheesy matter and phthisis; yet from our present knowledge of what constitutes tubercle, I cannot help interpreting all the forms of phthisis as of a unitarian origin. It is, after all, as Virchow himself says, only a matter of nomenclature. If we consider the presence of bacilli of Koch as the differentiating point between what is tubercular and what is not, we find that catarrhal and cheesy pneumonias are the most tubercular of all, because they contain, as a rule, more bacilli than any other forms of phthisis.

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[1] Cancer and tubercle are considered analagous lesions, and classed with tumors, by a number of pathologists. This fact would not make it inconsistent to call tubercle an inflammatory product, as the distinction between inflammatory processes and tumor formation is a purely arbitrary one. Virchow pointed out that the majority of tumors are purely inflammatory products (a statement antedated twenty years by Professor S. D. Gross). A few years ago I made the question of the ætiology of tumors a subject of careful personal study, which I yet continue, and I am forced to the conclusion that *all* true tumors are inflammatory products, and that no line of distinction can be drawn where the process which we call inflammation ends and where tumor formation begins.

Although cheesy pneumonia, like all forms of phthisis, remains commonly a local affection, it is seen on the autopsy table to give rise to miliary tuberculosis at least as often as any of the other forms of local tuberculosis.

We are then at present at the same standpoint in regard to the character of tubercle and cheesy matter as Lænnec (1819); and it is, indeed, perfectly reasonable to suppose that any cheesy matter found in a scrofulous person or animal is tubercular. Of course, it is evident that tuberculosis of the lung is usually accompanied by simple inflammatory products, such as organized connective tissue (chronic phthisis), or unorganized croupous and catarrhal exudates (predominating in acute phthisis), which may undergo rapid necrotic and purulent changes, resembling cheesy material. For the latter products the name "coagulation necrosis," as applied by the Heidelberg and Leipsic people, may be employed. Tubercle bacilli are commonly found in this coagulation necrosis. True tubercular cheesy matter should, I think, be considered only that product which is derived from the breaking down of previously well organized tubercle tissue.

I need not refer to the details of the manifestation of tubercle in the lung, as these are too well known. But I would like to remark here that those small whitish or gray nodules, usually of somewhat irregular shape, which are seen more or less densely scattered throughout the parenchyma of lungs affected by phthisis, are not miliary tubercles, but minute foci of broncho-pneumonia.[1]

These minute broncho-pneumonic foci take their origin from tuberculous matter disseminated by means of air-passages, as explained before. *Miliary* tuberculosis of the lung distributes itself by means of the perivascular lymphatics, is very rarely accompanied by catarrhal changes or hepatization, and rarely arises from a primary tuberculous focus of the lung itself; it is, as a rule, a part of general tubercular disease.

II.—THE PREDISPOSITION.

Having shown that for the production of tuberculosis we need a special soil, and that the irritant is only of secondary significance, some inquiry into the nature of this soil is necessary.

The question of the predisposition to tuberculosis, as it stands at present, must be considered from three aspects:

1. The clinical aspect.
2. The anatomical aspect.
3. The bacteridian or parasitic aspect.

The consideration of the clinical aspect of the predisposition to tuberculosis is invaluable, as it rests mainly on actual observation, on demonstrated clinical facts, and on conclusions drawn from statistics.

From time immemorial, a clinically well-defined condition of the system, known as the strumous diathesis in its various forms, has been recognized. This condition will be considered later on.

There are a number of ailments which, from the experience of clinicians, are known to have a great, direct or indirect, influence in the development of general tuberculosis and pulmonary phthisis; or are known to create conditions of the system that predispose it to this malady. Such are syphilis, inflammation of serous membranes, bronchitis, croupous pneumonia, diabetes, the exanthemata, especially measles and typhoid fever, deformities of the skeleton, rickets, cerebral and spinal diseases of various kinds, dyspepsia, the puerperal state, uterine diseases, prolonged nursing of children, onanism, change of climate, continuous loss of sleep, distress, etc.

That exhaustion, exposure, the deprivation of food, and other hardships of campaign life, etc., are prominent ætiological factors in the production of pulmonary consumption is learned from the accounts of military surgeons, who observed among young, robust soldiers a remarkable increase in the morbility and the mortality of phthisis, during and immediately after the close of a war. Such observations have been made in the Franco-Prussian and Turko-Russian campaigns. The fact that consumptive soldiers are not allowed to enter upon a campaign (certainly not in Germany and Russia) excludes here the probability of contagion.

Statistics also show the remarkable prevalence of phthisis in persons of certain occupations, such as stone-cutters, miners, cigar-makers, weavers, telegraph operators, book-keepers, and persons engaged in certain other occupations of a more or less sedentary nature. It is more natural to suppose that the disease or predisposition to it is created by the character and the conditions of the occupation, than that a contagion should affect preferably shoemakers, miners, or soldiers in the battlefield. Again, in most phthisical patients the beginning of the disease can plainly be attributed to an exposure, to a "cold."

On the other hand, there are pathological conditions or diseases which appear to prevent the development of phthisis and tuberculosis in general. It is an established clinical fact that phthisis is extremely rarely, if ever, associated with mitral heart disease; and, from my own observations, I believe that phthisis is rarely coincident with tumors. For the latter circumstance I can offer no explanation; nor is there any statement to this effect in literature. Rindfleisch has suggested that heart disease prevents the development of phthisis by inducing repeated slow congestions of the lungs, these congestions producing an overgrowth of the muscular tissue of the bronchioles and air-vesicles, which thus gains strength for repelling the exudates following inflammation.

If tuberculosis were depending upon a contagium for its development, neither heart nor tumor disease, nor any condition of the organism, could ever prevent its occurrence.

All the clinical facts above referred to prove definitely the necessity for a predisposition for the development of tubercular disease, and militate against the necessity of a contagium.

The anatomical aspect of the question—the morphology of the soil in which tubercle develops—is the most important aspect.

[1] See, in connection with this, the excellent studies of Wm. H. Mercur, from the pathological laboratory of the University of Pennsylvania, published only in abstract form in the *Phila. Med. Times*, July, 1883.

Beneke[1] tries to explain the disposition to tuberculosis by a disproportion between the size of the heart and blood-vessels and other organs to the bulk of the body.

Schottelius[2] made recently some interesting observations concerning the mode of termination of the smallest bronchioles and their relation to the lung acini in different animals. He found that in the carnivora the entrance of the bronchioles into the acini presented very small apertures, so that the air-vesicles were not easily accessible to irritants; while in the herbivora the terminal bronchial terminations were quite wide, thus permitting the free entrance of irritants. He states that in man the bronchial terminations congenitally approach sometimes those of the carnivora, and sometimes those of the herbivora. In the latter type, he believes to have found an anatomical explanation for the predisposition in some individuals to pulmonary tuberculosis.[3] Weigert, of Leipsic, (one of the most enthusiastic germ-theorists), properly remarks upon the observation of Schottelius, that it does not explain the predisposition, as the same animals will re-act, upon the introduction of the "poison of tuberculosis" into any other part of the body where the bronchioles do not come into play.

My own studies upon the minute anatomy of the tissues of man and of animals predisposed to tuberculosis, extended over a large amount of material, and gave results which, to my mind, satisfactorily explained this condition. These results I announced at a meeting of this Society in October, 1882.

The anatomical peculiarity observed in either man or animals, be it inherited or acquired, I first showed to be, briefly stated, as follows: All the tissues of the body approach somewhat an embryonal type; they are peculiarly rich in nuclei and young cells, and the lymph-spaces of the connective tissues are narrower, fewer in number, and show a great many more cellular elements in the scrofulous than in the non-scrofulous. So far, subsequent observations of others agree with mine. Objections are raised only as to the direct relation between these structural peculiarities and tuberculosis. Here I must state that I only suggested, and never asserted, the necessity of such a relation. It is quite possible that there are some other and more striking peculiarities in the morphology of scrofulous animals yet undiscovered. This much I can, however, reassert today: that tuberculosis usually ensues when a simple inflammation is set up by any kind of injury, in animals with the structural peculiarity which I have described; but tuberculosis cannot be produced in animals that do not have this structural peculiarity, so far as my experiments show, unless the injury is inflicted upon serous membranes.

For the details of my researches in this direction, I must refer to my first paper upon this subject.[4]

Koch asserts that the structural peculiarities of the tissues which I described can have no ætiological relation to tuberculosis, because an animal not possessed of such tissue peculiarity—the cat—is easily inoculable by tuberculous material. Here I must differ from Koch, as in my experience with cats this is not the case; and, again, Koch brings no proof for his assertion, and I am unaware that he, or anybody else, produced tuberculosis in a cat, except by inoculation into some serous cavity. That inoculations into serous membranes prove nothing for tuberculosis, as I have shown conclusively, Koch still seems to fail to see. But here is a way in which cats may become tuberculous, with or without the bacillus. In one instance we kept one of the cats in a close box, *deprived* of liberty, good air, the comforts of life, motion, and sufficient food; she also had been inoculated with diphtheritic material eight months previously, but had recovered. After the lapse of a year the cat was set free, but was accidentally killed, and was found to be affected by general tuberculosis in a high degree.

This, in my opinion, corresponds fully to the conditions in which a healthy young woman is placed, and finally becomes scrofulous, and then tuberculous, from a simple cold, after being the faithful nurse for a couple of years of a consumptive husband.

On the other hand, there is full reason to believe, as it is in accordance with experience, that young scrofulous persons, under proper conditions, may become normal individuals; *i. e.*, lose or outgrow the predisposition to tuberculosis. (I have dwelled upon this in my first communication on this subject.)

The scrofulous habit, and consequently also phthisis, may skip a generation, and does not invariably embrace all members of a family. It has been observed that parents may have at first healthy children without any vice, who grow old well; and subsequently the same parents, without being phthisical (but perhaps otherwise becoming deficient in health), may have other children that exhibit a full scrofulous habit. But even the reverse has been observed.

It would be highly desirable if physiologists would furnish some experimental observations on the circulation of the plasma in the lymph-spaces. This is, to my mind, a circulation or movement of the vital juices in the tissues, which, for the well-to-do of the individual, is of importance next to that of the blood. These important channels, the lymph-spaces, are known to regulate the blood-pressure, carry and breed (white blood corpuscles) food for the tissues, lubricate tissues and relieve the body, if any of its parts are damaged by injury of any character, of inflammatory exudates, dropsy, etc. These channels are nearly blocked up, nearly useless in the *scrofulous*, and hence cannot perform their functions; and thus modify materially the condition and the fate of the individual, in case of disease.

The term "*scrofulous*," which I retained for describing the above-stated anatomical peculiarity of animals and individuals, is as good as any other term; moreover it is known by all as designating the "predisposition" to tuberculosis. Scrofulosis should be called a *condition* and not a disease, as it has its

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[1] Die erste Ueberwinterung in Nordeney, Norden, 1882.

[2] Virchow's Archive, vol. xci, 1883.

[3] The method of investigating this condition is not without interest. The vesicular structure of the lung was injected, through the bronchi, with a resinous melted mass, which, on cooling, presented molds of the bronchioles, in connection with their characteristic infundibula and acini.

[4] *Loc. cit.*

(a natural) hereditary and widely distributed type in man, and its homologue in some normal animals (rabbit, guinea-pig, etc.). It must be remembered that the scrofulous individual acquires certain lesions, such as enlargements of lymphatic glands, cold abscesses, caries, long-standing catarrhs of various kinds, skin eruption, and certain deformities of bones, only under the influence of injuries, or of the same agencies which, in the non-scrofulous individual, lead to transient and curable affections.

Virchow designates simple, permanent enlargement (hyperplasia) of lymphatic glands, with or without cheesy change, "scrofulous," in contradistinction to "tuberculous" lymphatic glands, which contain miliary tubercle nodes (heteroplasia), and which also undergo cheesy change.

There is nothing called "scrofulous" or "scrofulosis," which by others is not also called "tubercle" or "tuberculosis." There are, strictly speaking, no scrofulous products, but only tuberculous products. The traditional term "scrofulosis" is variously used and interpreted, although it is not evident that any one means by it anything anatomically well defined.

Others take matters easier, calling everything *tuberculous* that contains tubercle bacilli, and calling scrofulous all cheesy matters in which bacilli are absent.

There is still a third aspect of this question, viz.: the parasitic or bacillary theory of the predisposition to tuberculosis. As I mentioned in the early part of this paper, Baumgarten, Mordiant and several others recently brought forward that not only tuberculosis but even the predisposition to tuberculosis is to be explained by the suceptibility of an individual to bacilli! Under this hypothesis, the inherited scrofulous tendency in individuals is created through the mediation of the bacilli. It is supposed that the bacilli or their spores may be conveyed to the ovum by the organism of the mother, or in utero by the spermatozoids of the father. Furthermore, they say, inheritance is to be explained in no other way than by a bacillary infection of the infant through the milk of the nursing mother, and by subsequent living together of children and phthisical parents. We may exclude such view altogether from consideration, as it has not been proven. Besides, it is not in accordance with facts from observation. It is as contrary to biological laws to accuse parasites for the transmission of a predisposition to tuberculosis, as it would be for that of epilepsy, etc. Hence we may dispose of such view as an unfounded, absurd hypothesis.

I am not opposed to the germ theory of disease, where it has its well-founded and proper application. Bacteridian studies have contributed largely to our knowledge of a certain class of pathological processes and lesions. But misinterpretations of the significance of bacteria; bacillary speculations, without occasion for them and without any proper application to the subject, are a check to the progress of medical science. The question of the predisposition to, and the cause of, tuberculosis, demands a great deal more of solid pathologico-anatomical and experimental studies; it can by no means be regarded as settled, and least of all through the discovery of a bacillus inhabiting necrotic tubercular tissues.

III.—TUBERCULOSIS, WITHOUT PREDISPOSITIONS, DUE TO INFLAMMATION OF SEROUS MEMBRANES.

For some years I felt much interested in the question whether or not simple inflammation of serous membranes could lead to tuberculosis in the non-scrofulous, that is, in persons who have no family history of tubercular disease; and I would like to ask the opinion and experience of the members of the Society upon this question. It is well known that there may be primary tuberculosis of serous membranes, producing secondary inflammations; and, on the other hand, tuberculosis secondary to adhesive pleurisy or peritonitis is also common in serous membranes. The general belief, however, is that whenever tubercular disease in either case occurs, if not secondary to phthisis or tubercular disease elsewhere, a strumous or scrofulous condition is required.

Traumatic injuries of joints are known to lead often to fungoid (tubercular) synovitis and general tuberculosis, occasionally in individuals with good family history. Simple injuries of the eye-ball (the anterior chamber as well as joints is lined by serous membranes), under conditions as above stated, have also been known to lead to tuberculosis, as recorded by Wolfe (*British Med. Journal*, March, 1882); Gradenigo (*Annale d' Oculistique*, 1870).

Dr. M. Litten,[1] of Berlin, was the first to publish some accounts which demonstrate that miliary tuberculosis may be caused directly and primarily by pleurisy and inflammation of other serous membranes in persons with no phthisical history, and without any cheesy masses being formed in any part of the body. In his (Litten's) experience this was particularly the case when there was a rapid reabsorption of the exudates in case of chronic pleurisy, or if repeated removal of the fluid of a hydrothorax or ascites by tapping has been performed. He records several well studied cases of that kind, accompanied by autopsy records. Litten's observations at no time, however, received their well deserved attention.

Not only clinically, but also pathologically, this part of the tuberculosis question is rather neglected. In text-books of pathology the occurrence of primary tubercle in adhesive bands is incidentally mentioned, but no special consideration is devoted to its ætiology and manifestations.

Upon the autopsy table I have repeatedly met with subjects with exquisite primary tubercular peritonitis, pleurisy, or pericarditis, which, upon inquiry into the history of the cases, failed to reveal any phthisical or scrofulous history. The products of these inflammations were often plastic in character, not unlike those of fungoid synovitis. The appearances sometimes present themselves particularly strikingly in the peritonæum; all the viscera may be glued together by plastic material into a solid mass. The omentum

[1] M. Litten, Sammlung Klin. Vortrage, No. 119. Ueber acute Miliartuberose. 1877. For further references see Wiener Med. Presse, No. 36, 1882; Charite Annalen, vol. vii, Berlin; Krankheiten der Respirations-Organe, in Virchow's Handb. der Spec. Path. und Ther., vol. i; Virchow, Geschwulste, vol. ii, p. 725, etc.; also, Formad, Transactions of the Phila. County Med Society, and of the Pathological Society, for 1882-83.

is usually retracted and matted together in a solid cord or mass, which, laying parallel with the transverse colon, reaches across the abdominal cavity, and may have a thickness of from two to four inches; the mesenteric and other lymphatic glands are usually normal, but sometimes in advanced cases may be much enlarged and more or less cheesy. The perfect absence of any cheesy focus in the body is, however, often a conspicuous feature in these cases.

Some pathologists deny the tubercular nature of these formations, and of the flat, nodular masses which cover the serous surfaces in these cases. It is true that fibroid changes predominate in these formations; but numerous tubercle nodules, with all the necessary attributes, epithelioid and giant cells, and necrotic changes, were plainly seen in all cases which I had occasion to examine. Secondary miliary tubercles of quite recent date are also found thickly strewed locally in these parts, and may or may not be seen in the lungs and other organs. As a rule, there are more or less ascites in these cases. My colleague, Dr. E. O. Shakespeare, has recorded similar cases, and Dr. Morris Longstreth tells me, also, that he had seen and studied such cases. Dr. Mitchell Prudden describes (New York *Med. Record*, June 16, 1883) an allied case.

In chronic adhesive pleurisy there occur similar primary tubercular formations in the organized plastic exudate, which in some cases gives rise to secondary (miliary) tuberculosis of other organs. The lungs may be perfectly normal in all parts, and show only peripherally, just below or bordering the pleura, some indurations of gray color made up of recent tubercle tissue. These young tubercle infiltrations are in some cases seen to have penetrated into the substance of the lung, like in a pleuro- or dissecting-pneumonia, directly from the old tubercular masses of the adjacent pleural membrane.

I have also examined several cases of plastic adhesive pericarditis, and found the plastic vegetations in this lesion to contain tubercles; two of these had coincident pleuritic lesions.

Cases which came under my observation during the last eighteen months—*i. e.*, since the opening of the bacillary campaign—were, of course, carefully examined for bacilli, and the results may be summarized as follows: Bacilli were found in most of the lesions, if the tubercular disease of serous membranes was accompanied by cavities and cheesy masses in the lung, or by tubercular ulceration of the intestines, and if cheesy changes in general were prominent; but no bacilli could be discovered, even after repeated and careful search, in any of the lesions of four cases of primary peritoneal and pleuritic tuberculosis examined. In none of these latter four cases were there any conspicuous cheesy changes in any organ, and no cavities or marked hepatizations in the lung, and no intestinal ulcers, although in two there was slight pulmonary miliary tuberculosis. These cases will be recorded in detail in a future publication.

I have also seen several cases of primary tubercular pleurisy and pericarditis, and a few of primary tubercular peritonitis, in the pathological institutes of Virchow in Berlin, and of von Recklinghausen in Strassburg. I questioned these foremost men of pathology concerning the ætiology of these lesions. They, as well as Rindfleisch, of Würzburg, told me personally their opinion, stating their firm belief that these lesions often directly originated from simple chronic inflammatory changes, without the agency of any cheesy focus, or any specific agencies whatsoever.

Birch-Hirschfeld also states, in his classical pathological work (page 183), that "nearly every exudative pericarditis and pleurisy leads to a local tuberculosis, if it takes a chronic course."

How often primary tubercular lesions of serous membranes occur in non-scrofulous persons, and whether this is the only form of tuberculosis in this class of persons, is, of course, a matter of speculation, until thorough statistics and careful studies are made in this direction. Nevertheless, it is a demonstrated fact, as I will show further on, that primary tuberculosis can be produced in the peritonæum of animals, like the dog, which are proved not to have any scrofulous tendency. I have seen this myself, and seen O. C. Robinson in my laboratory succeed in this experiment, by the introduction of simple irritants into the peritoneal cavity. Koch also never succeeded, even with the bacillus, in producing tuberculosis in the dog, except when using the peritoneal cavity or the anterior chamber of the eye (which is also a serous sac) as a point for inoculation.

Here is room for hypothesis. I would prefer to believe that tuberculosis could occur only in scrofulous persons, as this would better agree with the *scrofulous anatomy*. It is, however, possible that a scrofulous anatomy of the tissues may be artificially established by the blocking up of the lymph-spaces of the serous membranes, by fibrine and molecular *débris*, suspended in the serum which is being reabsorbed. This would then be a mechanical process, and not one of infection. If an inflammation occur in serous membranes, resolution becomes difficult through the peculiarity of the exudate. This is fibrinous mainly, and forming extensive, usually permanent organized deposits, it impairs the function of serous surfaces quite materially; the reabsorption of new exudates is probably, sometimes, entirely impossible. Thus conditions may possibly be created in serous membranes, not unlike those of scrofulous tissues, and simple irritants, perhaps the fibrine, may induce in them a similar reaction.

IV.—QUESTION OF CONTAGIOUSNESS. CLINICAL ASPECTS.

The idea of the contagiousness of tuberculosis is not new, and like other unfounded views in medicine, it has oscillated, like all fashions will, from one extreme to another for many generations. At present it is entertained by a number of scientists and by a part of the profession. This view has called forth from time to time, a number of researches whose results were either pro or contra. I will refer to these subsequently.

Of late, it appears that the belief in the contagiousness of tuberculosis has won considerable ground, not so much on account of accurate observation, as on account of Koch's discovery of the bacillus tuberculosis.

Another element which seems to have had an influence in this direction, is the fact that certain experimenters, formerly believing, from their own experiments, that tuberculosis was non-contagious, were led, later on, to change their opinions on account of the results of subsequent experiments. These latter experiments will, however, be shown not to be conclusive.

Before discussing the merits of the bacillus question, I would like first to consider the question of contagiousness from clinical grounds; and should it be proven that tuberculosis is not contagious, then the necessity for a contagium surely falls to the ground.

According to the observations of the most prominent clinicians who have paid special attention to this matter, there is not a single authenticated case of tuberculosis as a result of contagium on record. Among scores of experienced men who deny thus the contagiousness of tuberculosis it is sufficient to mention the names of Virchow, V. Recklinghausen, Stricker, in Germany; Gull, Williams, Watson, Paget, Humphrey, Richardson, in England; Bennet, in France; and Hiram Corson and Traillgreen in our own midst—all men of close observation, with ripe experiences reaching over thirty to fifty years.

The statistics of the large Brompton Hospital for consumptives, for thirty-six years, with regard to the resident officials, compiled by Dr. F. Williams (quoted after the *Lancet*, 1883), shows that of four resident medical officers, one of whom had served twenty-five years, none had any lung disease; of six matrons, none were consumptive; of 150 resident clinical assistants, eight became consumptive and five died, but in only one was the disease developed during residence at the hospital. Since 1867, of 101 nurses, only one died from phthisis, and that after leaving the hospital. Before 1867, six died, three of these of phthisis, but only one became so whilst resident, and she had a consumptive sister. She died thirteen years after first joining the hospital, but was not there the whole time. Of thirty-two gallery maids since 1867, none developed phthisis whilst at the hospital. Of twenty house porters, five died, but none of consumption. Non-residents: Of nine secretaries, three were threatened with lung disease, but recovered. Of twenty-two dispensers, seven died, three of phthisis, one while at the hospital. Of four chaplains, three died, none of phthisis. Of twenty-nine physicians and assistant physicians, eight died, none of phthisis. At the Chest Hospital, Victoria Park, there have been five resident medical officers during about the last fifteen years; all are alive and well. Two matrons, neither consumptive. There were two clinical assistants appointed every three months; none known to have developed the disease at the hospital. One nurse out of fifty or sixty in the last few years became consumptive while at the hospital, and she died after a year's illness.

An ingenious plan to decide the question of the communicability of phthisis was instituted by the British Medical Association by establishing the Collective Investigating Committee. This committee sent out questions relating to this subject to all the members of the Society. Of 1,028 replies received, 673 negatived the idea of a contagium, while 261 replies favored it. According to these statistics, there is a manifest majority in favor of the non-contagiousness of phthisis; yet such a plan is unsatisfactory, as the answers may be of unequal value, as their worth must be estimated in proportion to the experience and authority of the sender.

Not without interest is the observation of Prof. Corradi, of Pavia, who noted that out of 133 families in which he had cases of consumptives, in only 25 of the families were there more than one member of the family ill of that affection.

There is no proof whatever that tuberculosis is conveyed from person to person by contagion. Seeming exceptions to this assertion can almost always be accounted for in some other way.

The assertion that the wife may contract the disease from the husband I have pointed out, in a former paper, to be untenable; and I have also indicated that a predisposition to scrofulosis may be acquired from the unwholesome mode of life led, of necessity, by such individuals. Besides, it is established statistically that nearly one third of all deaths occurring in middle life are due to phthisis. In view of the frequency with which this malady occurs, intermarriage between scrofulous individuals may be almost as common as between non-scrofulous persons.

The view taken that children become scrofulous by contagion from phthisical parents, may be met by the fact that instances have occurred where a number of young children of phthisical parents were early removed from their homes and distributed among healthy families, and yet all, sooner or later, became phthisical.

Healthy persons have even been fed on bovine tuberculous material (which is considered identical with human tuberculous material, and have thrived on it, as is proven by the interesting feeding experiments made upon man and recorded by Schottelius, (Virchow's Archives, No. 91, 1883). The circumstances which led to this experiment were as follows:

In Würzburg, the sale of meat affected by pearl-disease or bovine tuberculosis is permitted, but, as some opposition to its sale once arose, a community of country people agreed to use exclusively tuberculous meat, on account of its cheapness and in order to prove that it was harmless. From Oct., 1867, to Nov., 1868, 49 tuberculous beeves, with well-pronounced lesions, were consumed by the people while they were under the supervision of the district physicians. In many instances the meat was eaten raw in consequence of habit. Ever since then, those people have continued the use of tuberculous meat, and thus far no bad results have been noticed; in fact, the record says that the people referred to are unusually healthy.

NOTE.—The concluding chapter will be furnished at once. It refers only to the diagnostic value of the tubercle bacillus, to the deficiency of the experimental proofs for the specific action of the bacillus, and to experiments proving that tuberculosis may be induced by non-tuberculous substances and simple irritants, in support of the statements made in the body of this paper.

THE DISTRIBUTION OF THE POISON THROUGH THE BODY AFTER DEATH FROM ARSENIC.

BY V. C. VAUGHAN, PH.D., M.D., AND J. H. DAWSON, PH.C.

Some months ago we reported in this journal a series of experiments which showed the diffusion of arsenious acid through the body when suspended in water and injected into the mouth and rectum after death. We have to report some experiments on the distribution of the substance after fatal arsenical poisoning.

Has arsenic any predilection of tissue?

Scolosuboff,[1] in 1875, reported that he had found, in experimenting upon animals, the largest amounts of the poison in the nervous tissue. He poisoned the animals with the arsenite of sodium, and after death estimated the amount of poison in 100 grams each of muscle, liver, brain and marrow. Representing the amount he found in the 100 grams of muscle by 1, that found in the same amount of liver would be represented by 10.8, that in the brain by 36.5, and that in the marrow by 37.3. Scolosuboff supposed that the arsenic replaced phosphorus in the lecithin of nervous tissue. This work has been frequently referred to in trials for murder by arsenical poisoning.

Ludwig,[2] in 1879, from experiments upon animals, and from the examination of the bodies of suicides, came to conclusions quite different from those of Scolosuboff. In all of Ludwig's experiments, he found a greater proportion of arsenic in the liver than in any other part of the body. In cases of acute poisoning, he found the kidneys also rich in arsenic, while the bones and the brain contained only small quantities. However, he mentions no case where a fatal dose had been taken in which he did not find some arsenic in the brain.

In the case of a suicide by acute arsenical poisoning, the results of Ludwig's analyses were as follows:

The liver, whose weight was 1480 grams, furnished 0.1315 gram of ammonio-magnesic arseniate, while the brain, whose weight was 1481 grams, furnished only 0.0015 gram of the same compound of arsenic. From 144 grams of kidney he obtained 0.0195, and from 600 grams of muscle 0.002 gram of ammonio-magnesic arseniate. In the bones, only traces of the poison were detected.

It will be seen from these figures that while the weights were substantially the same, the former contained about 90 times as much arsenic as the latter. It will be noticed, however, that Scolosuboff used sodium arsenite, which is more soluble and diffusible than arsenious acid.

Johnson and Chittenden[3] report the analyses of different parts of the body of Mrs. Riddle, poisoned. They found in the liver, which weighed 590 grams, 0.04788 grams of arsenious oxide (As_2O_3), and in the brain, which weighed 477 grams, only an unweighable trace. From these analyses, which embraced sections of nearly every part of the body, they calculated the total amount of arsenious oxide in the body to be 5.2261 grains. This was supposed to be a case of chronic poisoning.

In the case of Mary Stannard, whose death was supposed to have taken place shortly after the administration of the poison, Johnson found 83.23 grains of arsenious oxide in the stomach, liver and other internal organs, "while the brain contained a hardly perceptible trace of arsenic.

Of course, the unknown elements in all cases of arsenical poisoning are many and important. The exact amount taken, the amount removed by vomiting, and the time of administration are often unknown.

Johnson and Chittenden also made one experiment upon a large coach dog, which was poisoned by the administration of 6.5 grams of arsenious oxide in doses of from 0.1 to 2.5 grams per day for eight days. Twenty-four hours after the administration of the last dose, the animal was killed, and the different organs submitted to analysis. 100 grams of liver furnished 0.0010 gram of metallic arsenic, while the entire brain gave only a faint mirror.

Since these questions are of great importance in medico-legal investigations, and as the evidence furnished by the experimenters mentioned above is conflicting in some points, we determined to seek, experimentally, answers to the following questions:

(1.) In what proportion is the poison distributed to the various tissues in chronic poisoning by arsenious oxide?

(2.) What is the distribution in acute poisoning by the same poison?

(3.) What is the distribution in acute poisoning by the more soluble sodium arsenite?

Of course, these questions can be answered only in a general way. If the conditions of the experiment are varied, we should expect the results to differ in a greater or less extent.

The first experiment, representing a case of chronic arsenical poisoning was conducted on a small dog as follows: 685 grams of solid white arsenious oxide were given to the dog with meat during a period of sixteen days, in doses increasing from 0.335 to 1 gram. Seventy-two hours after the last dose was administered, the dog was killed and the following parts removed, and a quantitative estimation of the arsenic which they contained, made with the following results:

	Weight taken grams.	As_2O_3 Found.	Percentage of As_2O_3
Spleen	18	.00062	.00344
Kidneys	65	.00065	.00100
Liver	260	.01040	.00400
Lungs	95	.00040	.00042
Stomach and contents	152	.02530	.01664
Large intestine	42	.01520	.03619
Small intestine	70	.00030	.00043
Muscles from thigh	116	.00031	.00026
Muscles from abdomen	95	.00020	.00021
Heart	175	.00034	.00019
Blood	60	a very faint mirror	

[1] *Bulletin de la Société Chimique de Paris*, 24, 124.

[2] *Wien. Acadamie Auzeiger*, 1879. 181.

[3] *American Chemical Journal*, 2, 332.

Urine	41	.00060	.00146
Testicles	22	.00030	.00136
Brain	71	.00013	.00018
Total	1282	.05475	.00419

The second experiment represents cases of acute poisoning with arsenious oxide, and was conducted as follows. Two rabbits, one large, and the other small, were given arsenious oxide. To the larger, two grams were given, and to the smaller one gram was given. They died in about ten hours, and the following parts were removed and a quantitative estimation of the arsenic which they contained was made:

Large rabbit weighed 1520 grams.

	Weight taken grams.	As_2O_3 found	Percentage of As_2O_3
Kidneys	20	.00132	.00660
Liver	25	.00264	.01056
Lungs	7	.02640	.37714
Stomach and contents	170	1.62400	.95529
Large intestine	156	.23760	.15230
Small intestine	55	.00158	.00287
Muscle from abdomen	40	.00026	.00065
Muscle from thigh	47	A distinct mirror.	
Heart	10	.00124	.01240
Brain	5	A distinct mirror.	
Total	535	1.89504	.35421

Smaller rabbit weighed 562 grams.

	Weight taken grams.	As_2O_3 found.	Percentage of As_2O_3
Kidneys	8	.00053	.00662
Liver	32	.00164	.00512
Lungs	12	.01320	.11000
Stomach and contents	75	.66200	.88266
Large intestine	62	.05702	.09196
Small intestine	40	.00343	.00857
Muscles from abdomen	12	.00013	.00108
Muscles from thigh	12	A slight trace.	
Heart	5	.00026	.00520
Brain	4	A distinct mirror.	
Total	262	.73821	.28175

The third experiment represents cases of acute poisoning with sodium arsenite, and was conducted as follows: Two rabbits, one large and the other small, were given arsenious oxide in the form of sodium arsenite ($Na_3\ As\ O_3$). To the larger 2 grams were given, and to the smaller 1 gram was given.

Larger rabbit weighed 1,977 grams; died in 20 minutes.

	Weight taken. Grams.	As_2O_3 found.	Percentage of As_2O_3
Kidneys	19	.00330	.01736
Liver	65	.01548	.02381
Lungs	45	09240	.20533
Stomach and contents	70	.86100	1.23000
Large intestine	137	.04224	03083
Small intestine	37	.00489	.01321
Muscles from abdomen	55	.00198	.00360
Muscles from thigh	65	.00119	.00183
Heart	10	.00264	.02640
Testicles	13	.00026	.00200
Brain	5	.00053	.01060
Total	521	1.02591	.19691

Smaller rabbit weighed 464 grams; died in 4 minutes.

	Weight taken. Grams.	As_2O_3 found.	Percentage of As_2O_3
Kidneys	5	.00119	.02380
Liver	36	.00831	.02308
Lungs	8	.01056	.13200
Stomach and contents	31	.54120	1.74580
Large intestine	73	.05795	.07938
Small intestine	21	.00660	.03142
Muscles of abdomen	10	.00066	.00660
Muscles of thigh	10	.00053	.00530
Heart	2	.00158	.07900
Testicles	0.9	A slight mirror.	
Brain	3	.00019	.00633
Total	199 9	.62877	.31454

In these experiments the organic matter was destroyed by chemically pure hydrochloric acid and potassium chlorate. The arsenic was obtained by the Marsh test, and weighed in the metallic state.

It will be seen that in the case of chronic poisoning, for equal weights, the brain contained about one-sixth as much as the liver. In the cases of acute poisoning by the oxide, the amount in the brain was an unweighable quantity. In the case of poisoning with the arsenite, the amount in the brain is larger than when the less soluble oxide was given.

UNIVERSITY OF MICHIGAN, January, 1884.

GENERAL PARALYSIS FOLLOWING DIPHTHERIA. A CASE.

BY CHARLES WESLEY ROOK, M.D., QUINCY, ILL.

In the JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, vol. 1, page 649, two cases are noted of the rare sequel "General Paralysis After Diphtheria."

Having within my care, at the receipt of the above mentioned article, a little patient to whom had occurred this rare and distressing sequel, now that the case is terminated, I deem it a professional duty to give a report of the same.

For a brief and concise history of this pathological condition, its association, in the relation of cause and effect, with diphtheria, and for theories concerning its cause or causes, the reader is kindly referred to Trousseau's Clinical Medicine, vol. 1, p. 382, ed. 1882.

November 28, 1883, I was recalled to Stella P., æt. one year and six months, whom I had recently attended during an attack of diphtheria, together with four other members of the family, including the mother, who were also sick with diphtheria. By the close of the second week of treatment all the members of the family were convalescent, except one little girl, æt. three years, who died of diphtheritic croup at the close of one week of sickness.

On November 4, the subject of this report, though much emaciated, her pharynx being perfectly free of any diphtheritic membrane, was discharged. During the interval, elapsing between November 4 and November 28, I saw her twice and was much gratified at the rapid advances that she seemed making toward a complete recovery. When called, Wednesday, No-

vember 28, her mother said that the Saturday previous Stella did much running and playing in the yard; while playing, she was frequently observed to lie down on her back, cry for a few moments, and strike with her feet as if they were giving her pain, then getting up would go on playing as usual. On Sunday and Monday her gait became very unsteady, and on Tuesday she was unable to walk. Wednesday, when placed upon her feet, could stand for only a moment, then would sink to the floor. By inclining her body forward, while supporting her upon her feet, she would make a feeble attempt to walk, but was unable to raise her feet and bring them forward. Examination proved the muscular tissue of the legs to be soft and flabby, the skin of a pale blue color, and the feet and legs, though covered with two pairs of stockings and kept close to a stove, were extremely cold. At the same visit it was noted that the muscular and coördinating powers of the hands and arms were apparently unaffected. The appetite was poor, but the child seemed very thirsty, and at nearly every attempt to quench her thirst, she would strangle, an attack of coughing would be induced, and the greater portion of the liquid would be returned, through the nostrils, to the cup. Her pharynx was completely free of any diphtheritic deposit. The actions of the bowels and bladder were regular. Albuminuria is said to be present quite generally, but I was unable to get a specimen of the urine for examination. The respiration and pulse were somewhat accelerated.

December 1, was hastily summoned, and learned that Stella had had a convulsion, following an attack of coughing, occurring soon after eating dinner. This, I think, was due to some article of food getting into the air-passages. She was very fretful and restless, especially of nights, not even permitting a light blanket to cover her limbs. December 4, I called. Found Stella sleeping. Her left axilla and ilium were approximated several inches nearer than normal, because of a tonic contraction of the muscles of the left side of her body. The muscular power and voluntary movements of the hands and arms were becoming impaired.

Though seen daily, no marked change other than the gradual loss of power in her hands and arms was noted until December 10, when I called and found the surface of her body hot and dry; pulse 160 per minute; the respiration rapid, feeble and shallow; the pupils widely dilated; was unable to raise either hand from off her chest, and in addition to the other symptoms noted, was a tonic contraction of the cervical and dorsal muscles, so that the weight of her body was supported by the occiput and sacrum. During the continuance of this cervical and dorsal muscular rigidity, which was four days, the limbs were completely relaxed, so that we could extend or flex them into any desired position.

Also during this interval of four days there were visual disorders. The pupils remained dilated so that she did not seem to see or recognize her friends. Convergent strabismus was also present, apparently equally affecting each eye, for upon attracting her attention by the exhibition of some plaything, she would fix the object first with one, then with the other eye. December 13, she was attacked with purulent ophthalmia; the discharge, which was profuse, thick and yellowish, continued for three days, then gradually disappeared At this time she had a very troublesome cough, the attacks of which seemed to be precipitated by her attempts to swallow liquids. December 17 she was attacked with a slight diarrhœa, which was probably reflex in nature, as she was at this period cutting her superior and inferior canine teeth. The paralytic symptoms were slowly disappearing. December 21, was still improving; drinks without becoming so frequently strangled; her cough less troublesome, and she sleeps well of nights. December 28 she made her first attempt to crawl and walk since she became paralyzed. January 1, 1884, continued her efforts at walking, but because of her weak and emaciated condition the attempts were but feebly executed. Her appetite was increasing, and the excretory functions were normal. January 9, convalescence being established, my little patient was discharged.

In order to complete this report, I called January 28, and found Stella so completely recovered as to leave not a trace of her recent serious illness.

The plan of treatment adopted was the following: For the indications present when first called this combination was ordered:

℞ Tr nucis vomicæ sp. chloroformi, āā. ʒss; acidi nitro-muriatici dil., ʒj; inf. gentianae, q. s. ʒiij. M. S.: A teaspoonful three times a day.

This prescription was continued until December 9. December 3, being very fretful and restless, she was quieted with the $^1/_{32}$ of a grain of morphia sulphatis. This amount was occasionally exhibited to quiet her restlessness throughout the course of her illness. December 8 and 9, two applications were made, of four minutes each in duration, of a mild induced electrical current. December 10, the paralytic symptoms being greatly augmented, and because of the inability to swallow, the nux-vomica preparation and the application of electricity were discontinued, and in their stead, rectal injections of equal parts of spiritus frumenti and milk were ordered. December 13, the $^1/_{50}$ of a grain of nux-vomica was ordered twice daily. For the ophthalmia, was directed thorough and frequent cleansing with cold water. December 14, to relieve the cough the following was given: ℞ tr. chloroformi, ʒj; acidi hydrobromici, ʒij; syr. scillæ, ʒiij; syr. simp., q. s. ʒiv. M. S.: One-half teaspoonful every six hours. December 18, the nux-vomica was increased to the $^1/_{50}$ of a grain three times a day. December 19, as a general tonic, the following was administered: ℞ tr. ferri chloridi., ʒss; extractum malti, q. s., ʒij. M. S.: One teaspoonful three times a day. The three latter prescriptions were continued until convalescence was attained.

AN AGED PHYSICIAN.—It is reported that Dr. Jakinoriez, of Jarvszinka, in the Government of Kiew, Russia, has just died at the age of 107, and that he was able, until nearly the close of his life, to superintend the working of an extensive practice.

MEDICAL PROGRESS.

MATERIA MEDICA AND THERAPEUTICS.

ANÆSTHESIA BY A MIXTURE OF MORPHIA, ATROPIA AND CHLOROFORM.—Dr. C. Aubert communicates to the *Société de Biologie* (*Comptes Rendus*) the results which he has obtained in his surgical practice by preceding the use of chloroform by the injection of

Morphia chlorhydrate.....10 centigr.
Atropia sulph..... 5 milligr.
Aqua dist.....10 grams.

He uses 1½ grammes of this solution 20 or 30 minutes before beginning the inhalations. In children he uses not more than 1 gramme. He has used this method in more than 60 cases, and, as far as the atropia is concerned, has never seen the slightest disagreeable symptom, no dilatation of the pupil, perhaps a little dryness of the throat, and considers that these agents furnish great security against the danger of cardiac syncope following chloroform inhalations. The advantages claimed, are: First, the security. Second, the greater rapidity with which anæsthesia is produced. Third, the absolute quiet of the patient. Fourth, the facility of awakening. Fifth, the absence of subsequent discomfort and vomiting.

As to security, he refers to one case where in an aged subject there existed adhesions of the pleuræ, extensive pulmonary emphysema, marked mitral insufficiency, interstitial nephritis and purulent cystitis, as proven by the autopsy three weeks later; this method was followed, and there was no perceptible influence upon the pulse. It is true that there was cyanosis which required the use of artificial respiration for two minutes. He accepts, therefore, the views of MM. Morat and Dastre that atropia diminishes the excitability of the pneumogastric.

As to the rapidity of its action; out of 40 patients 4 came under its influence in 1 minute, 5 in 1½ minutes, 12 in 2 minutes, 6 in 2½ minutes, 4 in 3 minutes, 2 in 4 minutes, 4 in 5 minutes, 1 in 6 minutes, 1 in 7 minutes, and 1 in 10 minutes—giving a mean of 3 minutes.

NARCOSIS BY CHLOROFORM, BY ETHER, AND BY NITROUS OXIDE.—Neudörfer (in the *Deutsche Zeitschrift für Chir.*) investigates the above, and comes to some highly important conclusions, which may be thus summarized. 1. Air cannot hold more than 20 per cent. of its bulk of chloroform-vapour at the ordinary temperature and pressure. 2. Chloroform acts as a poison upon the red blood corpuscles, but only slowly, even upon corpuscles at rest, whilst those circulating through the lungs are not long enough under its influence to be destroyed. 3. It has been found by Bernard, Gréhant, Hoppe-Lezler, and Hermann, that hæmoglobin has a greater affinity for carbon monoxide than for oxygen, and for nitrous oxide than for carbon monoxide. In other words, in presence of an excess of carbonic oxide the blood cannot absorb oxygen, and in presence of an excess of nitrous oxide the blood cannot take up either carbonic oxide or oxygen. Hoppe-Lezler and Gréhant think that if the weakest of these gases (in their affinity for hæmoglobin) be previously present in excess, it hinders absorption of the gases of stronger affinity. As to this, the authors think that it does not hinder this absorption, but that the latter are thus rendered innocuous, and also that the affinity of hæmoglobin for oxygen is not constant, but varies within certain limits. 4. If to an individual, whose hæmoglobin has a relatively weak affinity for oxygen, we give to narcosis a mixture of 20 volumes of chloroform-vapour to every 80 volumes of air, he breathes a mixture holding 16 per cent. of oxygen, instead of the usual 20 per cent. in the air, and it may happen that he may take up little or no oxygen, and the chloroform may exert its fatal effect. This is a simple and sustained method of explaining death by chloroform. 5. Now, arguing by analogy, since the very poisonous carbonic oxide can be rendered innocuous by the previous presence of other gases in excess, it is to be presumed that in a mixture of chloroform-vapour and oxygen, the chloroform could not exert its evil effects upon the blood corpuscles. 6. There would be no danger of oxygen poisoning, for the quantity given would be within the limits of the variations of oxygen taken up in nasal respiration. 7. The bad after-effects of chloroform would be done away with. 8. Twenty per cent. of chloroform-vapour would be unnecessary, since three to five per cent. are quite enough. 9. As to ether, it can be mixed up with oxygen up to forty-three per cent., but it cannot compete with chloroform, because it possesses no constant chemical constitution, and its behaviour to human tissues is more prejudicial than that of chloroform. 10. The mixture of nitrous oxide and oxygen acts equally as well as the chloroform and oxygen mixture. 11. Oxygen is easily administered by Simonson's apparatus, 12. Narcosis thus caused is absolutely devoid of danger.

MEDICINE.

MORPHIA HABITUES.—Dr. Benjamin Ward Richardson (*Lancet*) read a paper on this subject before the Medical Society of London on December 10. His remarks were directed especially to the habitual hypodermic injection of morphia, the dose varying from less than a grain to fifteen grains per diem. He gave the summary of symptoms in a case of daily injection of three grains and a half for a period of two years as being as follows: (*a*) Torpidity in the action of the liver; (*b*) abscess forming at or near the site of puncture; (*c*) tendency to local induration, and (*d*) a free excretion from the bronchial surface; (*e*) difficulty, occasionally, in swallowing morsels of small substances, as if the object were arrested in its passage owing to its becoming enveloped in a layer of tenacious mucus; (*f*) a tendency at times to violent perspiration, more often present when the morphia has been almost entirely excreted, or when the system has been much debilitated; (*g*) various perversions of sensations; (*h*) occasional difficulty in articulation, like that resulting from alcoholic excess; (*i*) impotency; (*j*) difficulty of breaking off the habit owing to craving for the narcotic, and symptoms of

debility when it was left off; (*k*) mental exaltation and depression, with tendency to exaggerate dangers; (*l*) excessive excitability of the imagination when reading, *e. g.*, works of fiction; (*m*) lessened sensitiveness and receptivity with inclination to apathy, varied, it may be, with brief periods of mental exaltation.

The more serious subjective and objective symptoms induced by deeper indulgence in this habit were defined as: Increased appetite for the morphia; loss of will power; confirmed apathy from the ordinary toxic effects of opium; failure of all the mental faculties, together with general failure of voluntary and involuntary muscular power. Diminished vitality, with bending of the body and the signs of premature old age.

PULSATING LIVER.—Dr. Walter G. Smith (*Dublin Journal of Medical Science*) reports a case of this character in a soldier 35 years of age, who suffered from lung trouble and some anasarca of the legs not higher than the thighs; was a free drinker. The skin of the face and hands was dusky in color, and there was visible pulsation in the jugular veins, which were of immense size. Albuminuria; liver dullness 2½ inches below the ribs on the right side, and to within 2 inches of the umbilicus in the middle line; margin readily felt. A distinct pulsation of the liver was visible in the epigastrium, extending over the right and left lobe. Subsequently the liver increased in size, and the lungs became engorged, while at times a triple cantering sound was heard over the apex of the heart, and the systolic murmur became more distinct. The area of cardiac dullness appeared to be enlarged, and the apex beat was visible in the sixth intercostal space. In support of his proposition that the phenomenon was principally the result of a retrograde venous wave from the right ventricle, he mentions:

1. The expansile sensation communicated to the fingers when placed upon the liver.

2. The duration of the pulsation. It continued after the ventricles ceased to act.

3. As the condition of the patient improved under treatment, the hepatic pulsation became less and less. If the hepatic pulsation were due altogether to direct shock, the contrary would be the case, for steadier and more forcible action of the heart would materially increase the pulsation.

He asks the following questions: Ought pulsation of the liver to be regarded as evidence of regurgitation through the tricuspid orifice? How does the regurgitation of blood into the hepatic veins in sufficient force to visibly agitate the liver, sometimes occur? And why is it not more frequently observed in old-standing cases of mitral disease?

Respiration aids the venæ portæ in accelerating the blood-current through the liver, and this is favored anatomically by:

1. The trunks of the hepatic veins, which almost pierce the tendon of the diaphragm, to join the inferior vena cava.

2. The hepatic veins cannot collapse when subjected to atmospheric pressure (because of their parenchymatous attachment).

3. During the inspiratory descent of the diaphragm the vena cava is straightened, and the course of the hepatic veins made more direct. Conversely, during expiration the venous trunks between the liver and the heart are shortened, and the opening in the diaphragm is diminished, counteracting the tendency to regurgitation. But if, under any circumstances, regurgitation of blood from the right heart takes place, here is a condition especially favorable for communicating to the liver as a whole any pulsation caused by reflux into the inferior vena cava and large hepatic veins. The degree to which the liver can yield is regulated by the elasticity of the enveloping capsule of the liver and of its peritoneal covering. Hence, inflammatory thickening of the capsule is an obstacle to pulsation.

SURGERY.

CATHETER FEVER.—Sir Andrew Clark (*Lancet*) on a recent occasion brought this subject before the Clinical Society of London, but as the constitution of that society did not permit such abstract questions to be discussed, on December 17 he addressed the Medical Society of London, giving his views more in detail, and recounting the history of medical observations and records of catheter fever from 1800 down to the present day. He offers the following propositions:

First, that about middle life in men perfectly healthy, or with no discoverable evidence of disease, except perhaps, and even that not always, a low density of urine, the commencement of the habitual use of the catheter is sometimes followed by fever of the remittent type, which often ends in death, and that for the fatal issue in such cases no adequate structural explanation can be found.

Secondly, that it is important that such a fever, arising in the midst of apparent health from such a seemingly small cause, and leading so often (as it certainly does) to a fatal issue, should be well and widely known, lest death should take the friends of the patient by surprise, and arrangements necessary to the welfare of a family should be left unmade.

Thirdly, that although it is well known that in persons affected with renal disease, or with chronic gout, or with grave disorders of the general health, the commencement of habitual catheterism is attended with peril to life from secondary fever, the fact that this fever may arise in what seems to be good health, and without the mediation of any visible structural lesion, issue in death, is not well known.

Fourthly, that this fever is neither distinctly uræmic nor distinctly pyæmic; that although having some of the characters of each, it has all the necessary characters of neither; that probably it begins in the nervous system; that probably the disturbance of the nervous system reacts in the first instance upon the general metabolism of the body, and in the second instance upon the secretory organs, beginning with the kidney; that the effect upon the kidney may consist either in structural alterations of the kidney, or

in alteration of the constitution of the blood, that dynamic condition of its constituents in the renal vessels essential to the elaborative action of the secretory cells thereof; and lastly, that the concurrence of these conditions may be, and often is, enforced by septic reabsorption into the blood.

Fifthly, that a more complete knowledge of this variety of fever, and of the conditions of its origin, maintenance and increase may, at least we may hope, lead to a material diminution of its mortality; and that even now, by treating in a serious manner entrance upon cather life by great temperance in the use of foods and stimulants, by rest, warmth, and by other general means, such mortality may be possibly considerably diminished.

Two questions of a practical kind arise out of this study of the history of catheter fever. The first is: As there is no account of the fever following in cases where narcotics or anæsthetics have been used, may it not be that the fever is capable of being cut short by the administration, on entering upon habitual catheterism, of narcotic or anæsthetic remedies? The second is: Assuming the presence of the fever, and seeing that quinine has signally failed in controlling it, what are the drugs to be employed on such occasions? And what is the sort of hygienic management to be followed, especially in respect of food and alcohol, which are so variously used on such occasions, in order that the fever may be brought, if that be possible, to a successful ending?

Sugar as an Antiseptic Dressing.—Dr. Fischer states that Prof. Lücke, of Strasburg (*Centralblatt f. Chir.*), has since May last been making trials of sugar as a pulveriform antiseptic. He has used it mixed with equal parts of naphthaline or with a fifth part of iodoform, enclosing it in gauze bags, which are fixed over the wound after the application of sutures. When the skin is defective, the sugar is strewn over the wounded surface. The wound has been disinfected during the operation by means of a 1 per cent. sublimate solution. The dressing may remain on the part from a week to a fortnight, until the sugar becomes dissolved, the secretions from the wound diffusing themselves equally throughout the sugar. If, however, the sugar is applied too thickly (*i. e.*, more than half a centimeter), it forms into lumps. The wounds thrive under the sugar, the dressing emitting no bad smell nor exhibiting bacteria. The granulations are well developed, having no inclination to bleed, and cicatrization proceeds rapidly. In wounds united by suture, primary union has always been obtained. The experience thus far gained justifies the recommendation of further trials of a remedy so easy to obtain.—*Med. Times and Gazette.*

The Use of Hodge's Pessary in Fractures of the Lower Jaw.—Dr. W. J. Naismith (*Lancet*) describes a fracture of the lower jaw at the symphysis, with a transverse wound two inches in length over the mental protuberance. The fragments of the jaw were freely movable, and it was desirable to apply an apparatus which would fix the bone in place immovably, and at the same time allow the wound to be dressed. Accordingly, a Hodge's pessary was brought into use, by bending it so as to allow the chin to protrude through its ellipse. One bar was moulded so as to support the fracture anteriorly, the other steadied it from below, while the rounded ends afforded admirable lateral pressure on each side, at a point in front of the angles of the jaw. To the rounded ends of the pessary tapes were sewn, two on each side, over the padding, and secured over the head, or to a fillet, and around the neck by small buckles. For fractures of the inferior maxilla at or near the symphysis, with or without wound, the Hodge's pessary seems well adapted. It can be bent to fit any size of jaw, and in the qualities of comfort, lightness and coolness compares very favorably with the solid, cumbrous appliances included under the head of moulds.

TOXICOLOGY AND MEDICAL JURISPRUDENCE.

A Rapidly Fatal Case of Arsenical Poisoning. Dr. David W. Finlay gives a case in the *Lancet* of a man fifty-one years of age, in moderate health, who swallowed, by careful computation, twenty-six grains of arsenious acid; was seen and prescribed for within a half hour; had not vomited, but the stomach pump was used freely, with no benefit, for he died in just one hour after swallowing the fatal dose. His symptoms were generally those of collapse; nothing at the post-mortem indicated any particular poison, until the contents of the stomach were analyzed.

A recent non-medical French writer (A. Cels. Bibliothicaire de l'Université de Bruxelles) in reviewing the *Manuel du Bibliothecaire*, of M. J. Cousin, takes occasion to emphasize the fact that the form of the Index catalogue of the Library of the Surgeon-General's Office, U. S. A., will be universally adopted by librarians as fast as it becomes known to them. He reviews this mode of cataloguing, giving to it the name of "*Le système Billings*;" and concludes by addressing a request to the *savant* Dr. Billings, that he publish a new manual, which will, beyond doubt, replace all of those which have been published up to the present time.

The Wooden Leg in the Gallic-Roman Period.—Our friends the archæologists are exciting themselves in France over the discovery on pieces of ancient pottery two delineations of figures wearing each a wooden leg. M. Emile Riviere, in the *Gazette des Hopitaux*, has an interesting article on the subject which is well illustrated; one of the figures is that of a male playing on the harp, or lyre; this is supposed to be Apollo, and the writer quotes M. Maximin Legrand as follows: "Have you ever heard, my dear readers, that the grandfather of medicine had his leg broken? As for me, I am absolutely ignorant of the fact. I have heard of the death of Adonis and of the mutilation of Atys, as names which, among the ancients, personified the sun, and of whom the death or debility, always followed by a resurrection or cure, symbolized the conditions of universal life, or simply the order of the seasons. But a wooden leg! of what can it be the symbol?"

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THE EDITOR of this JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information, should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor

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WAS the New York State Medical Society "extruded from membership" in the American Medical Association, or did she take herself out by *her own act?*

In another column of this number of the JOURNAL will be found a letter from New York signed "Vindex," and written by one of the most intelligent and influential of the members of the New York State Medical Society who were active leaders in the movement that resulted in the adoption by that society of a new code of ethics as a substitute for the National Code in 1882. We invite particular attention to this letter, as it is a fair example, both in its temper and its carelessness of statements concerning facts and events, of what has been written and said by the party the writer represents during the last two years. The leading object of "Vindex" in writing his letter appears to be the correction of the impression conveyed in the leading editorial in this journal of January 26 ult., that the New York State Society had voluntarily cut herself off, or severed her connection with the American Medical Association by her own deliberate act. On this point he uses the following language: "It (the State Society) did not. It was *extruded* from membership in that body (the American Medical Association) by the action of its Judicial Council at St. Paul in 1882, which action the Association, as a body, assumed by adopting the report of said Council."

In further proof of such "extrusion," he says the State Society sent delegates to the meeting of the National Association at St. Paul; that he, as one of the delegates, presented his credentials to the Registration Committee at an early hour on Tuesday morning, June 6, and was told, in a not very polite way, that the Judicial Council had determined, at a meeting held on the previous day, which would have been on Monday, June 5, "to throw out the credentials of the delegates from the Medical Society of the State of New York" If our friend "Vindex" had ever taken the trouble to examine with reasonable care the constitution and by-laws of the American Medical Association, or even read the official record of the proceedings of the Association at St. Paul, he would not have committed the mistake of representing the Association as *adopting* the report of the Judicial Council, when the decision of that tribunal is final without further action of the Association. Nor would he have gone so far astray in his memory as to represent the registration officer as saying that the Judicial Council had decided on the previous day to "throw out credentials," instead of what the officer doubtless did say, namely, that protests had been lodged with the Registration Committee, and that under the by-laws the credentials and protests must be reported to the Association and referred to the Judicial Council before the registration could be effected.

Neither would he have represented the Judicial Council as acting upon a matter on Monday, which could by no possibility have come before it until after the papers had been presented, by the chairman of the Registration Committee, to the Association on Tuesday morning, and by that body referred to the Judicial Council. If Vindex, or any of our readers, will turn to the volume of Transactions of the Association for 1882, pages 59 and 60, he will find that the first meeting of the Judicial Council was convened at ten o'clock on Tuesday morning, June 6, at which officers were elected, and action taken on a case concerning the Nebraska State Medical Society which had been referred to the Council the year previous, and on nothing else. The next meeting was at five o'clock P. M. of the same day. At that meeting, the protests and papers in reference to the right of the Medical Society of the State of New York to be represented by delegates in the Association, were received from the Permanent Secretary, the same having been referred by the Association during its general session in the morning. They were examined, together with a certified copy of the new code of ethics adopted by that State Society at its annual meeting in February preceding. A final decision was deferred until the third meeting of the Council, held at 8:30 A. M. on Wednesday. At that meeting, after further consider-

ation, the Council arrived at the following decision, which was reported to the Association near the close of its general session on the same day:

"Having carefully considered the Code of Ethics adopted by the New York State Medical Society at its annual meeting in February, 1882 (as furnished by the Secretary of said Society), the Judicial Council finds in said revised Code provisions essentially differing from, and in conflict with, the Code of Ethics of the Association, and therefore, in accordance with the provisions of the ninth By-law of this Association, decide unanimously that said New York Medical Society is not entitled to representation by delegates in the American Medical Association." See Transactions, Vol. 33, p. 33.

The ninth by-law referred to in the above report is in the following words:

"No State or local medical society, or other organized institution, shall be entitled to representation in this Association that has not adopted its Code of Ethics; or that has intentionally violated or disregarded any article or clause of the same." See Transactions, Vol. 33, p. 629.

With this plain and explicit rule, as a condition of representation in the Association, published in every annual volume of Transactions for more than twenty years; and the equally direct statement of "Vindex" in his letter, that the Medical Society of the State of New York had actually adopted in February, 1882, a new code as a substitute for that of the American Medical Association, can any intelligent reader entertain a doubt as to whether such action of the State Society necessarily deprived her of all right to further representation in the national organization. The decision of the Judicial Council simply declared the fact ascertained, that the State Society of New York had "violated or disregarded" the Code of Ethics of the Association and in *consequence* of such action was not longer entitled to representation in that body. That the action of the State Society in repudiating the Code of Ethics of the Association directly in violation of the by-law quoted, caused the EXTRUSION, is too plain for argument.

Instead of correcting anything contained in our leader of January 26, the letter of "Vindex" contains a full acknowledgment of the fact, that the work of revising and altering the National Code of Ethics was entered upon by the State Society of New York in 1881, and completed at the annual meeting in February, 1882, without the slightest regard to the fact that said National Code had been compiled and adopted by a national organization composed of her own delegates, in association with delegates from the State societies of all the other States, and who had consequently an equal interest in it with herself.

Though a member of the committee that did the *revising*, he says he "did not hear any discussion in regard to the American Medical Association." The truth is that "Vindex" and his co-laborers in the work of ethical revising, had given very little attention to the national organization for years, and appear to have had very little knowledge of its constitution or conditions of membership.

Under the specious pretense of reforming and liberalizing the ethical rules governing consultations, they proceeded with their work without fairly realizing the undesirable position in which they were placing their State Society in its relations to the Association and all the societies of other States, until they appeared at St. Paul as delegates from a society whose right to representation had been forfeited, in part at least, through their own influence.

Their position is an awkward one, and we can appreciate the underlying chagrin that prompts their irritability when the plain facts are brought to their notice, but instead of floundering from one false position to another, until new State and local societies are formed all over their State, and their representatives admitted to the national organization, and to fellowship with the societies of other States, why not manfully acknowledge the primary error, undo the objectionable work, resume the former relations of harmony with the national and State organizations of the Union, and then propose whatever changes may be desired, and let them be considered and acted upon by the representatives of the whole profession, where such consideration rightfully belongs.

What the letter of "Vindex" says about the framing and promulgating an "iron-clad oath," is fully and correctly answered in THE JOURNAL for January 19, 1884, and is too absurd to require further notice. The resolution which he proposes for the Association to adopt, as an act of reconciliation, is more curious than important. The credentials of a delegate consist of a certificate by the proper officer of some medical organization that the bearer A. B. has been duly elected or appointed a delegate to the American Medical Association. To be "duly accredited," it must be from a society or organization entitled to representation under the constitution and by-laws of the Association. What the laws regulating the practice of medicine have to do with such certificate it is difficult to conceive.

DR. ELISHA HARRIS, of New York, died a few days since in the city of Albany. He had been many

years connected with the departments of health and public charities, and was one of the most eminent sanitarians in this country. A more extended sketch of the life and labors of the deceased will be found under the head of Necrology in the next number of THE JOURNAL.

MISSING NUMBERS OF THE JOURNAL.—We have calls for a few copies of Nos. *two* and *six* of THE JOURNAL that we are not able to supply at present. We again request anyone who may have duplicate copies of these numbers to re-mail them to this office; and if any one has received and paid for these numbers who do not care to keep them, we will pay four times the subscription price for their return in good order to us.

WINTER CHOLERA AND HOG CHOLERA.—Dr. F. Dedolph writes from St. Paul, Minn., suggesting that the so-called winter cholera, which has been noticed in various places during the last few years, is caused by the use of lard and oleomargarine prepared from hogs affected with the disease known as hog cholera. This letter contains a printed slip showing that he made the same suggestion three years since. But as he gives no facts or investigations bearing upon the subject, we give his suggestion here instead of his letter.

SOCIETY PROCEEDINGS.

KALAMAZOO ACADEMY OF MEDICINE.

The Kalamazoo District Medical and Surgical Association has recently been incorporated in accordance with the laws of the State of Michigan, under the style "Kalamazoo Academy of Medicine." The meetings of the Academy are held in the city of Kalamazoo the last Tuesday of each alternate month of the year, beginning with January. It invites to its membership all regular practitioners of medicine or surgery in Southwestern Michigan. The Secretary would be glad to receive the names of any who desire to unite with the Academy. The meeting held January 29 was a most profitable and pleasant one for all. The officers elected for the year 1884 were: President, Josiah Andrews, of Paw-Paw; First Vice-President, Foster Pratt, of Kalamazoo; Second Vice-President, O. F. Seeley, of Clina; Secretary and Librarian, H. B. Hemenway, of Kalamazoo; Treasurer, O. B. Ranney, of Kalamazoo. Board of Censors (term expires in three years): H. O. Hitchcock and W. L. Worcester, of Kalamazoo; (term expires in two years) Foster Pratt and J. W. Fiske, of Kalamazoo; (term expires in one year) L. D. Knowles, of Three Rivers, and O. F. Burroughs, of Galesburg.

THE SUFFOLK DISTRICT MEDICAL SOCIETY.

SECTION FOR CLINICAL MEDICINE, PATHOLOGY, AND HYGIENE.

ALBERT N. BLODGETT, M.D., SECRETARY.

December 12, 1883.—The meeting was called to order at 8 o'clock by Dr. R. T. Edes, chairman.

The first paper of the evening was read by Dr. E. O. Otis, entitled

THE USE OF TOBACCO AMONG BOYS.

Dr. H. I. Bowditch, in opening the discussion, said that it is high time that some measures were adopted to stop, or at least to restrain, the use of tobacco. For thirty years he has continually found a certain number of patients who have presented the symptoms of grave functional disturbance of the heart, for which no cause could be found except tobacco. There is no organic cardiac lesion, but it is a nervous weakness. This condition was very commonly observed during the late war. Nearly all the soldiers smoked, and many gradually acquired what was appropriately named the "tobacco heart." The reason why this condition prevailed so extensively at the time mentioned, is no doubt found in the fact that tobacco enables men to endure hunger, cold, hardship, and privation better than they could without its aid. The active, exposed muscular life of the army enabled the soldiers to withstand the injurious effects of this substance better than would be the case in a sedentary occupation, or where one lives as a recluse. A moderate use of tobacco may unquestionably be continued for a long time, and probably indefinitely, without harm. There are few people in adult life who would derive any injury from three cigars daily; but the danger comes from the fact that the number is increased to ten or fifteen, and sometimes even to twenty-five in a day. Nearly all boys now smoke cigarettes to an inordinate extent, and it is painful to look forward to their future condition.

In habitués of tobacco we frequently find errors of digestion, sometimes of most refractory character, though many persons commence to smoke from the supposition that the digestion is benefited by the use of tobacco. There is no question upon the fact that mental disease may be induced by the poisonous action of nicotine; and many smokers who are not insane have been rendered exceedingly nervous and irritable by the prolonged use of tobacco, and immediately give unmistakable evidences of absolute derangement if the customary indulgence is for any reason withdrawn. The duty of physicians would seem to be to endeavor to restrain the use of tobacco to an amount which shall not be injurious; for no attempt to stop its employment would probably be successful. Above all we should seek to restrict its use among students and youths generally. Punishment, such as expulsion or suspension from school, should be attached to disobedience, and all proper means shuld be employed to protect our scholars from its influence. The medical profession should take a decided position upon this important subject. Not only tobacco, but all stimulants should be restrained to a

limit within which they can do no harm. Tea and coffee should doubtless be included in the category of agents capable of harm. When in Paris, in 1854, Dr. Bowditch found the favorite *café noir* to be such a nervous stimulant that he was able to work all night without the least feeling of fatigue. On returning to America, however, he found himself unable to follow his professional duties with his accustomed energy, but felt a great sense of lassitude and depression. He now reduced the use of coffee to once a day; but after three or four months he found himself subject to attacks of palpitation and cardiac pain, which recurred with considerable regularity. Dr. Jackson examined the heart, but could detect no organic lesion, and advised abstinence from coffee, upon which all symptoms at once disappeared. After a time the use of coffee was again commenced; whereupon the previous disturbances of the heart were soon reproduced. The harmful agent was again abandoned for 15 years, when being once more in Europe, coffee was resumed in small quantity, but within a short time the old symptoms all recurred in a more intense form. Since that time no coffee has been taken, and no further trouble has been experienced. The human system seems never to fully recover after once being thoroughly saturated with any of these subsances. The same is true of alcohol.

Dr. George B. Shattuck thought it a too common habit, in speaking of these matters, to make the *abuse* and the *use* of a substance synonymous. It is also common to form general conclusions in regard to the effects of stimulants and narcotics from personal and individual experience, and to apply to our neighbors the rules we find necessary for ourselves. It is possible to be indiscreet and to commit excesses with beefsteak as with tobacco, and the individual should decide what *his* system requires. The subject of discussion before the Society, however, the speaker suggested, was the use of tobacco by *boys;* and in regard to this he felt sure that the members present would cordially agree with the paper of the reader, Dr. Otis, and with its conclusions that the use of tobacco by the immature, even in moderate quantities, is injurious and undesirable.

Dr. Harlow observed that if tobacco retards development during the years of growth, it must continue to be a harmful substance even after the body has reached its full growth, and would work injury at any time of life. If it irritates the heart in a boy, it will do so to some extent in a man. President Quincy was accustomed to advise students against the use of tobacco or stimulants while engaged in their collegiate course.

Dr. Marcy stated that when a surgeon in the army he had observed occasions when tobacco was a godsend to the soldiers in aiding them to endure hardship and privation, and cold and hunger. There are some organisms to which it may be of questionable benefit, but it is a serious injury to any growing person. There is no doubt that a popular vote would be largely against the general use of tobacco.

Dr. Prince thought a distinction should be made in the form in which tobacco is employed—whether in cigarettes, or cigars, or a pipe. Cigarettes probably make a more lasting impression upon the system than cigars, and are harder to renounce. There is a popular impression that the principal injury comes from the paper containing the tobacco. This is not true. In cigarette smoking the vapor is inhaled, and coming in contact with the delicate mucous membrane of the air passages, its active properties are at once absorbed, and produce an immediate effect, which can be perceived to the tips of the fingers. The succeeding sensation is one of ennui, malaise, indolence, and muscular hebetude, which soon becomes burdensome unless the stimulation be renewed and prolonged by a fresh cigarette.

Dr. Marcy said that the question of the *form* in which tobacco is used is really important, if we are to judge of the *effect* from the quality of the substance employed. Many cigars are made from refuse of all kinds, which is pressed into shape and inclosed in a genuine tobacco leaf for a wrapper, while cigarettes more generally contain pure tobacco, particularly when they are made by the smoker as required for use. Certainly, as far as wholesomeness and cleanliness are concerned, cigarettes must be ranked as far preferable to cigars.

Dr. Langmaid stated that the most important questions in cigarette smoking are these: Does the inhalation of the vapor induce any disease of the mucous membrane with which it comes in contact? Does the smoke act upon the mucous membrane simply as smoke, or from the fact that it contains nicotine? Does smoking increase naso-pharyngeal catarrh? To these an affirmative reply must be given. If the mucous membrane is irritated, smoking invariably makes it worse. Will smoking produce a catarrhal state when none exists, or awaken a new one in a patient who has been cured of the disease? Observation teaches that it can do this. There is an increased secretion from the membrane, and all the other features of a catarrhal condition. Singers almost invariably abstain from smoking on those days upon which they expect to sing.

Dr. Bowditch added that he will not treat a patient for sore throat unless smoking is abandoned for the time. He has, however, never observed any affection of the bronchial tubes as indicated by râles or other physical signs, which could properly be laid to the influence of tobacco.

Dr. Cutler asked if any distinction was to be drawn between smoking and chewing. He stated that a teacher told him that students became much more stupid after smoking than after chewing tobacco. Dr. Mussey has made the same observation in regard to the mental depression from tobacco.

Dr. Edes remarked, in closing the discussion, that the subject had been presented with great clearness and in a very unprejudiced manner. One of its excellent features was the absence of exaggeration upon a topic which is generally discussed in an eminently partisan manner. There is no doubt something to be said on both sides.

Dr. George M. Geddes presented the next subject of the evening, as follows:

THE TREATMENT OF FOLLICULAR TONSILLITIS.

Follicular tonsillitis is a self-limited disease, which runs its course in a certain limited time, presents a sequence of definite and successive phenomena, and finally returns to a normal condition in about the same interval of time, whether any active treatment be employed or not. Any treatment, therefore, is directed chiefly to minor points, such as (1) the rapidity of relief; (2) the time when the patient may return to business; (3) the convenience and comfort of the patient as far as the method of treatment is concerned, and the ease with which the remedies can be applied. The disease invariably presents itself in one of two types. First, there is a type in which the follicles are stuffed full, with swelling of the parts, general malaise, high temperature, and great prostration. The malaise lasts from twenty-four to thirty-six hours, and subsides entirely without treatment. I generally give some quinine and a gargle, because it seems necessary to give something in order to minister to the feelings of the patient and his friends, but the cure would take place equally well without any treatment whatever. In the second type there is high fever, considerable pain, prostration, and high pulse. The patient is in bed and appears very sick. The general symptoms far outrank the amount of disease in the throat. This variety lasts from three days to a week or fortnight, running through a similar series of changes, excepting that they are sometimes more pronounced, as are observed in the milder variety of tonsillitis. The tonsils gradually return to a state of health, and the patient is cured just as in the type before mentioned. I have seen advised and have tried some of the nastiest things which can be made in the form of gargles. There is no danger attending the disease itself, there is no necessity for treatment of any kind, and, above all, there is no excuse for anything in the shape of disagreeable treatment of this disease by dirty and nauseous mixtures. The question of treatment rests only on the plea of shortening the disease and benefiting the patient. My plan is about this: Where the temperature is elevated to about 102° F or 103° F, I first give the patient a *sweat*. I do this by means of aconite. I prescribe five drops of the tincture, and direct that one-third be taken immediately, one-half of the remainder in half an hour, and the rest in an hour. I then give five grains of Dover's powder, and cover the patient warmly up to the neck. When sweating commences I give all the water the patient desires for two hours. At the end of that time the sweating is generally pretty profuse. The patient is dried and allowed to get on the dry side of the bed, and is directed not to rise until the next day. My apology for the sweat is that it always relieves the sore throat, back-ache, pains in the limbs, and nausea. The relief to the throat from aconite is very great, so much so that the patient can generally take milk with comfort, which he could not do before. The next day there is only slight further treatment for the throat. I generally direct the patient to gargle with hot water; and by hot water I mern *very hot*, as hot as the patient can bear it. Hot water is preferable to cold. There is no treatment before the sweat, and no call for anything further after the gargle. Each time the hot water is used the patient feels better. There is do excuse for carbolic acid or any other medicament. No germicide can be used in sufficient strength to affect bacteria without being injurious to the throat. After the treatment above described there is generally constipation, which is relieved by a seidlitz powder; and I usually prescribe quinine as a tonic, to be taken for some time afterward in small doses.

My reason for introducing the subject of the treatment of follicular tonsillitis is, that many physicians do not understand that it is a self-limited disease, and will do very well if let alone. They prescribe some disagreeable or disgusting gargle, which the patient often will not use, and sometimes cannot, and which it is practically impossible to employ in the treatment of children, who do not know how to gargle, and who resist anything of a disagreeable nature in the throat.

In a recent number of a Philadelphia medical journal I find an article by a man of that city upon this subject, and I desire to call the attention of the Section to the plan of treatment there advocated. If anything more nonsensical in theory or more disgusting in character can be put together, I do not know what it is. First, the throat is gargled or swabbed with a solution of equal parts of tincture of chloride of iron and water. This is repeated every two hours. The patient is confined in the bed, and not allowed to get up. Ten grains of quinine are given, and repeated in four hours if the fever has not subsided. This treatment is followed up until the throat symptoms disappear, and is then replaced by a tonic. The writer adds, "cases usually recover in about a fortnight." Think of a child gargling the throat with a 50 per cent. solution of chloride of iron! And the rest of the treatment there advocated is fully as unnecessary as it is painful to the patient and burdensome to the friends. I claim that such treatment is not only useless but unjustifiable in any disease which is bound to get well anyway, and which we can influence by measures equally effectual and less troublesome.

Dr. Liebmann observed that in tonsillitis there is, according to the books, general malaise but no rigor; while in diphtheria there is rigor. It is frequently the case that diphtheria comes on very insidiously, and commences as a tonsillitis, often confined at first to one tonsil, but afterward extending to the other, and being followed by the usual local signs and constitutional symptoms of diphtheria. It is a mistake to suppose that the onset of diphtheria is always announced by a rigor. Tonsillitis may be preceded by a chill, while diphtheria often supervenes without any such symptom.

Dr. Langmaid said: The great interest in follicular tonsillitis consists not in what the disease *is*, but in what it is *not;* in other words, in the absence of any exanthem, or parenchymatous disease, or deep-seated infiltration of the throat or other parts, and in the freedom of the patient from any symptoms of a more serious malady. We are never sure that the sore throat, the follicular disease, may not be the local manifestation or the initial symptom of some

grave constitutional affection, and we should be ever on our guard. The patient should be watched, and the same precautions observed in mild cases which would be necessary in a far more serious disease. It is not certain that the particular process of sweating has any advantage over the aconite alone, or over the old time remedy for sore throat, nitrate of potash. The pain in the muscles is often relieved by the administration of salicylate of soda. All the older authorities speak of colchicum and guiac, and they evidently considered the pains as of rheumatic character.

Given a case such as is described by Dr. Garland, how can the physician be certain that the superficial inflammation is not to be followed by abscess of the parts? Dr. Garland does not, probably, mean to indicate that *all* cases should be looked upon as absolutely innocent. How can he know that, instead of the creamy deposit, there may not supervene a thick deposit and all the signs of a grave exudative disease?

Any one who has had the care of diphtheria must feel anxiety upon this point. A subsequent visit should always be made, in order to be certain that no dangerous condition has supervened upon the simple tonsillitis. After the exudation there is sometimes serous infiltration of the tonsil, and the symptoms of diphtheritic infection appear.

The observer in Philadelphia is not so far wrong, for he doubtless felt the apprehension of some grave complication, and was striving to ward off more serious evils.

Dr. De Blois remarked that there is tonsillitis and *tonsillitis*. In many cases the patient is unable to open the jaws; and when the throat can be examined, there may be very little observable change in the tonsil. Scarification of the gland is often a most beneficial mode of treatment.

On motion of Dr. De Blois, it was then voted that the further discussion of this subject be postponed until the next meeting, as an invited guest of the Section, who was present, had a very interesting case to show to the members.

The report was divided at this point, and the rest published one week later.

THE TREATMENT OF CLEFT PALATE.

Dr. H. A. Baker, by invitation, exhibited a patient for whom he had contrived an improved appliance for cleft palate.

Dr. Henry J. Bigelow remarked that it is quite exceptional that staphyloraphy alone without an obturator does much service to the speech. It is not difficult to get a union of the fissure, or of most of it, but the flaps are usually so tense afterward from the contraction of the dissected surfaces behind the palate that the play of the palate necessary to articulation is prevented. I have, therefore, usually recommended patients to get one of the varieties of artificial palate or obturator, whether inflexible, like Suersen's, or flexible, like Kingsley's, and to submit only to so much operation as may be necessary to keep it in place. After an operation, patients expect to talk at once, but they cannot do so. A long education is needed to show any considerable improvement in alternating the mute and nasal consonants. As a rule, there is no improvement of speech at first. Fifteen years ago I devised a short series of exercises in articulation for a patient I operated upon. It begins with the only consonant which a patient can usually best articulate, namely, "*t*" in "*tar*," and gradually leads to the rest, constantly referring to the acquired "*t*" as a point of departure. Here it is, as it was the printed for the private use of a patient:—

"The great difficulty in pronouncing correctly with a cleft palate is in distinguishing the nasals from the mutes; thus, p and b from m; pap or bab from mam; t and d from n; tat from nan; k and g (hard) from ng.

"'Tar' is well pronounced by most beginners with an obturator. When the beginner can pronounce 'stark' and 'car' he has the key to most of what here follows. The above words should be practiced carefully; not 'start' and 'tar,' but 'stark' and 'car,' and should be spoken loudly, or, as the elocutionists say, 'exploded.'"

1.	tar	artar	kar	arkar	kar
2.	kar	arkar	arkgar	kgar	gar
3.	kar	arkar	arkdar	kdar	dar
4.	kar	arkar	arkpar	kpar	par
5.	kar	arkar	arkbar	kbar	bar
6.	kar	arkar	arklar	klar	lar
7.	kar	arkar	arksar	ksar	sar

Practice all the above with the following vowels:—

8. o as in coke.

Thus, instead of kar, akar, etc., ko — oko — oklo — klo — lo.

9. a (long) as in cake.
10. i as in kite
11. e as in keep.
12. u as in suit.

13.	kar	arkar	arngar	arkar	arngar	kar	ngar
14.	tar	artar	arnar	artar	arnar	tar	nar
15.	par	arpar	armar	arpar	armar	par	mar
						bar	mar
						dar	mar
						sar	rar

(Practice reading loudly from a book.)
"February, 1870."

Dr. Geo. F. Grant spoke as follows: In the physiological treatment of cleft palate, there are many points which require the most careful consideration. I have listened to the paper of Dr. Baker with great interest, because the subject is one to which I have devoted much time and attention, and during the past 14 years have treated over 70 cases of congenital cleft palate. While I may claim to have succeeded in almost every case in securing a good articulation, there have been great variations in the degree of success, as well as in the length of time which is required for the patient to master the art of distinct articulation and enunciation.

At the commencement I made the mistake of expecting too much from the appliance unaided by proper direction to the patient in its use, that is to say, after adjusting the palate it was my practice to give the patient an exercise in sounds, simply requesting him to follow me in their pronunciation. This plan did not prove satisfactory in all cases, and I now often wonder how so many did so well under so imperfect a system of training.

It soon became plain to me that patients of this class required to be taught the art of correct articulation technically, just as we are taught any other art

which depends upon accurate technique. This fact applies also to the sister art of enunciation, and although there is quite a wide difference between these two factors of speech, they are so closely allied in this particular branch of study that they may be considered as one in speaking of them in this relation.

I found that this class of patients is almost universally deficient in the management of the breath; that in the use of the voice they speak directly from the lungs without any check at the larynx as is the case in normal speech, and where this fault does not exist, they attempt the closing of the nose by using the depressor muscles of that organ, and thereby disfigure the face greatly.

While this fault exists, the development of correct habits of speech is impossible; for many if not all the sounds of the language require for their proper enunciation that the air column be under the most perfect control. For example, most patients are quite unable to give the sounds of those letters which are classed as labials, with any force whatever after being taught the method, simply because they do not have the power of sustaining a sufficient pressure of air. The proper thing to do is first to teach them to control the breath. It is best to give them a tube of some description, and teach them to blow through one end while the other end is immersed in water; the bubbles which arise are a good indication of the progress which is being made. They are then taught to check the air in the throat by the tongue, and by distending the cheeks make a reservoir of the mouth which furnishes air to the tube while the lungs are being filled through the nostrils, so that a continuous stream of air passes through the tube. This, many of you will recognize as the method of using the blow-pipe, familiar to chemists and artisans. I know of no plan which so rapidly gives the patient that control of the breath so necessary to the first steps in articulation. This practice gives tone and strength to the lips and cheeks, the muscles of which I have observed to be much weaker than in persons of normal formation. I have seen many patients who could apparently distend the cheeks as well as any one, when in reality there was little or no pressure upon the inside, and the act was a fictitious one, as the cheeks would not sustain percussion.

After I have taught a patient to direct a column of air through the lips and to control its passage, it is much easier to instruct him in applying those modifications to the air current which constitute articulate speech. These modifications are taught singly, and the proper position and conditions of the organs employed in the production of a sound are ocularly demonstrated, first, because the patient can more quickly understand what he can *see;* secondly, because it is of the greatest importance that the best skill should assist in developing the work which the appliance is designed to accomplish.

I have only time to say that this plan or system which has been slowly and practically developed has for its basis the reduction (by analysis) of speech to its simplest mechanical principles, and no effort has been spared to bring the mastery of those principles within reach of the most ordinary intelligence. I do not agree with much that has been written and said regarding the classification of the sounds in the language. But have one remark to make regarding the most common error which, as I believe, can be shown by a little reflection. It is a common belief that patients of this class must struggle with the consonants, while the fact is that with a few exceptions the vowel sounds are much the most difficult to teach them, because the vowels are more dependent upon the quality of the voice as a whole than upon any position or local arrangement of the organs of speech, consequently a patient may be technically correct as to articulation and have a most unpleasant enunciation, due entirely to faulty location of the vowel sounds. That is, a patient may be able to give the sounds of simple single letters or elements, and yet be unable to converse or read distinctly, because the voice has not received attention.

Few of these patients have made use of the anterior portion of the mouth in their efforts to articulate. Their whole mechanism has been misdirected to an almost vertical line with the larynx, so that if the same line were followed with a normal palate the voice would pass through the nares behind the palate, instead of being thrown upon the anterior portion of the soft palate and the whole of the hard palate, and then by reflection through the lips, as should be the case.

If you have followed me to this point, you will observe that I have first taught the patient that he must have the air to make the sounds under his control; secondly, he has been taught how to employ it in conjunction with the organs of speech in forming the parts of speech; now it remains to show him where to put it. We all agree that those sounds, or tones rather, which have the best penetrative power and are most clearly enunciated have a high or sharp pitch; then the next thing to consider is how is it produced. Why are some men's voices so much clearer in tone than others? Some of it is due no doubt to anatomical causes, the height of the dome of the palate for instance. But still there are physiological reasons. The man who enunciates best, and whose voice has purity and clearness of tone, makes it better still by careful study and training. He brings the focus as far forward upon the palate as it is possible to do; the sound wave from the larynx is projected as far upon the anterior portion of the palate as possible, by holding the head in an erect position and opening the throat, thus bringing the tongue forward. It is this very point which it is important to impart to a patient with cleft palate, and which is necessary for him to master in order to complete the work which an artificial palate makes possible. No enunciation is good without it; no articulation can be secured which will repay the trouble unless careful attention is given to these points, and I have seen so much accomplished by these methods that it seems almost a possible thing to train a patient into a fair articulation without any appliance. Two quite remarkable cases have come under my notice. In both the fissure was quite large, in one patient extending through the soft palate, in the other through the soft and half

way through the hard palate. In both cases the articulation was remarkably good, and either by accident or training the method in one was almost faultless, so that I advised the patient to let well enough alone.

A word about appliances. Dr. Baker's appliance seems to me to be the addition of a hinge to a modification of the Suersen obturator made by Dr. Kingsley, of New York. The original Suersen obturator was a solid bulb of hard rubber, so shaped as to retain contact with the divided palate under all conditions, and at the same time close the opening into the nares by the contact with the superior constrictor of the pharynx. Dr. Kingsley's improvement consisted in making the bulb hollow, and placing it so as to slide upon a bar until the most effective position was determined, and then securing it in the desired position. The method of making the bulb hollow, and the conclusions at which he arrived regarding the comparative value of the Suersen and Kingsley systems, may be found in the book on Oral Deformities, by Norman W. Kingsley.

Dr. Kingsley is of the opinion that a soft rubber appliance is best to acquire articulation with, while the hard rubber might be inserted with good results after articulation had been acquired, and I coincide with that view; for when a case of cleft palate has been brought to a successful result, and the palate is then removed, the patient is in a position analagous to perforate palate.

The articulation exists, and only requires mechanical aid, so that whatever meets that requirement suffices.

There are objections to soft rubber as a material, but it has proved of much benefit as an aid in giving the best results in acquiring good articulation, and in that respect surpasses anything which I have seen. I do not think Dr. Baker did it justice in his remarks upon it, for he must have known that if it was properly made, the die in which it was made was a permanent mould, which rendered the vela capable of easy and limitless duplication, and so easy of adjustment that it could be sent to any part of the world, or, if necessary, made or duplicated by any dentist who would follow the directions which accompany it.

The cleanliness depends upon the individual, as does the cleanliness of anything worn about the person.

The best results are dependent solely upon skillful manipulation, and no appliance can be successfully adapted by an unskilled hand. There can be no fixed rule to follow; each case presents new features, which must be met as they appear. My preference is for the soft rubber, because it more readily fulfils the requirements, and because it has done better work than any appliance which I have yet seen, though I have seen a few cases in which the hard rubber obturator had proved very beneficial.

I will not intrude further upon your time than to say that one of my greatest complaints against the Suersen principle has been that it seems almost impossible to obtain a clear voice with it, a fact which I have attributed to its thickness obstructing the posterior portion of the nares, and in Dr. Baker's appliance I cannot see that the hinge obviates that difficulty.

The patient was then introduced, and the appliance could be seen in the mouth, and, by causing him to swallow, the relation of the instrument to the wall of the pharynx and to the alæ of the velum palati could be easily observed. It was evident that in the act of deglutition no communication between the nasal cavity and the pharynx existed, the posterior part of the obdurator being raised up by the superior constrictor muscle of the pharynx, and pressed firmly against the posterior wall of the pharynx at the line of the naso-pharyngeal junction. The patient now removed the appliance, when an enormous fissure was disclosed, extending from a point near the incisor teeth through the entire hard palate and velum. Upon being asked to read, it was quite impossible to understand what the patient was saying. After replacing the appliance the same sentences were read, and every word was spoken with great distinctness, and could easily be understood by every person present. After this the patient was asked to read some new selection, and opening a book at random, commenced as follows: "Physicians generally agree that the moderate use of wine is beneficial." At this point the audience, pleased with the improvement in speech, and possibly desiring to express approbation of what was read, burst into applause, and appeared to be perfectly satisfied as to the usefulness of this instrument in cases of cleft palate. The patient remarked that he found the greatest comfort and solace in the fact that with the appliance in his mouth he was able to smoke, while without it he could not draw the vapor of the tobacco from the pipe into the mouth. With it, also, he can whistle, which he cannot do without it.

On motion of Dr. Marcy, it was voted that the thanks of the Section be rendered Dr. Baker for his important communication of original studies upon a most difficult subject, and demonstration of ingenious mechanical appliances for the remedy of cleft palate.

On motion of Dr. Prince, a vote of thanks was tendered to the retiring chairman, Dr. George B. Shattuck, for the zeal and fidelity with which he has conducted the deliberations of the Section during the past two years.

Adjourned at eleven o'clock.

DOMESTIC CORRESPONDENCE.

NEW YORK LETTER.

To the Editor of The Journal of the American Medical Association:

Dear Sir:—In your issue of January 26, 1884, is a leader headed "An Ethical Symposium," in which you make animadversions upon the action of the Medical Society of the State of New York in amending its Code of Ethics in 1882.

The argument of your leader is obviously intended to propagate the idea that the Medical Society of the State of New York voluntarily and intentionally seceded from the American Medical Association. It did not. It was extruded from membership in that body by the action of its Judicial Council at St. Paul

in 1882, which action the Association, as a body, assumed by adopting the report of said Council. These and some other facts in the matter should be stated again, as your leader was evidently written by some one who either does not know them, or refrained from using them.

In 1880, New York State passed an act regulating the practice of medicine within its borders. The year following, the Medical Society of the State appointed a committee to revise its code. I was on that committee, and did not hear any discussion, if any occurred, in regard to the American Medical Association. The revision was made to conform, in its text, to the provisions of the revised statutes of the State, and the claims of public policy, and was done under the advice of some of the best legal minds of New York City. The code, as revised and reported, was adopted by the New York State Society without even a verbal amendment, in 1882.

After this action, the society sent delegates to the meeting of the American Medical Association at St. Paul. The first session of the meeting occurred on Tuesday. The writer, at an early hour on that day, presented his credentials, and was told in a rugged, not to say impolite manner, that the Judicial Council of the American Medical Association had determined, at a meeting holden the previous day, to throw out the credentials of the delegates from the Medical Society of the State of New York. The writer, on two occasions during that session of the Association, asked the President of the Association for an opportunity to be heard in regard to the action of the Judicial Council. That official declined to give him any such opportunity. It was obvious that the American Medical Association was in the grasp of a few men who, through the Judicial Council, exercised their unchecked sway. The writer, not desiring to create tumult in such a body, contented himself with quietly taking part in the affairs of the meetings in his capacity of permanent member and delegate from the Medical Society of the County of New York.

In 1883 the Medical Society of the State of New York did not elect delegates to the American Medical Association. At that meeting the Judicial Council of the Association, in the exercise of its assumed high prerogative, framed and promulgated an iron-clad oath intended to keep out of the meeting any one who would even call in question, the wisdom of its Code, or if he did call it in question to place him in so doing in the attitude of having subscribed to a code which he intended to defy or subvert.

I have no doubt that the State of New York will again be in the American Medical Association, but not till the American idea of federation is exemplified in its laws and practice.

The passage of some such resolution as the following would, no doubt, bring about so desirable a result, namely: "Resolved, that no delegate, otherwise duly accredited, shall be excluded from a seat in the Association, whose credentials are plainly consistent with the laws regulating the practice of medicine in the State or Territory from which he comes." That would carry out the American idea of federation, and throw back upon the States the responsibility of legislating for the regulation of the practice of medicine. Some prejudices would have to be set aside to make way for such freedom, but in the end those States would exercise, by common consent, the greatest influence for good in the Association, in which the profession had ripened best under the influence of good schools; aided by good laws. Every State and Territory would, as in the American system, have the moral and political weight it was entitled to, and there would be something more in the American Medical Association than a doubtful "flavor of nationality." I am, dear sir, yours respectfully,

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FOREIGN CORRESPONDENCE.

FROM PARIS.

PARIS, January 16, 1884.

In opening his course of lectures as Professor of Clinical Obstetrics, to which chair he had recently succeeded the late Prof. Depaul, Prof. Pajot pointed out the necessity of adjoining a chair of gynæcology to that of obstetrics, as he said that both branches were so intimately connected that he could not understand how such a desideratum was allowed to exist, and he believed that France stood alone in this respect. Under existing arrangements gynæcology is not taught at the medical schools, nor is it officially recognized as a part of the medical curriculum in this country, the consequence is, a great number of students leaving the medical schools are completely ignorant of this most important branch. It is true the students obtain a theoretical knowledge of the principles of gynæcology, but when examined clinically they display an amount of ignorance which should not be allowed to exist. Simple gynæcology could not be taught at the lying-in hospital, as patients were admitted only to be confined, and they left it on the ninth day after confinement, unless they were not considered in a fit state to do so. Professor Pajot opened his new course under the most favorable auspices. He has promised great improvements and stated that as far as lay in his power he would raise obstetrics from its present obscure and cirumscribed position to which a narrow-minded prejudice has hitherto assigned it.

There is much ado about the consumption of American pork in this country, as a short time ago a slight epidemic of trichinosis occurred at Crépy-en-Valois, which was attributed to the use of American pork, but it was ascertained that the pork was absolutely French, as the pig to which the origin of the epidemic was traced, was killed in this country. The epidemic, however, was soon got under, and did not extend beyond the above-named town. In September last an epidemic of trichinosis broke out at Emersleben, near Halberstadt in Germany, and Professor Brouardel was deputed by the French Minister of Commerce to proceed thither to study and report upon the epidemic. He was accompanied by Dr. Grancher, whose conjoint report was submitted to the Minister of Commerce, and as it was demonstrated

that the trichinosis was only caused by eating pigs' flesh in a raw or uncooked state, a practice so common in Germany, there was little fear of the malady gaining ground in France, as almost every article of food was submitted to heat or to some culinary process. Moreover, the origin of the German epidemic was traced to an English pig that had been lately imported, and as there was nothing else to incriminate American pork, the Minister of Commerce thought proper to remove the prohibition on that article, as it constituted the principal food of the working classes, particularly among the peasantry.

The question was again brought before the Chamber of Deputies by M. Paul Bert, as well known as a politician as a physiologist, who moved that the recent decree rescinding the prohibition should not be carried into effect. This was carried by a large majority at the Chamber, so that the first decree still remains law. The Minister of Commerce, however, not being satisfied with the unceremonious manner with which his orders have been treated, re-transferred the matter to the Academy of Medicine, where it has been freely discussed.

M. Bouley, the well-known veterinarian, proved that the interdiction of American pork was a mere delusion; that during the twenty years prior to the interdiction American pork was freely eaten in France, and yet he had not heard of a single case of trichinosis in it. But the ludicrous part of the affair is that since the prohibition was decreed in 1881, the importation continued all the same; only instead of coming direct to this country, the American pork passed through England and Belgium, where no case of trichinosis had been heard of. The deduction, therefore, is that the nocuity of American pork is only another scare. The French must always have one to keep their restless minds occupied, and if they have no real subject for agitation they must invent one, and this applies as much to special or general questions as it does to their politics.

The question in dispute has been consigned to a commission, and perhaps nothing more will be heard of it for some years. Meanwhile the very people whom M. Paul Bert pretends to protect (and they are the great majority of the population) will be the greatest sufferers in a commercial as well as a sanitary point of view, for as long as the prohibition lasts they must be deprived to a certain extent of a most precious and economical article of food.

At a recent meeting of the French Society of Temperance a discussion took place on the propriety of adding strychnine to alcoholic drinks, as recommended by Dr. Luton of Reims, with the view of counteracting the ill effects of these liquors. Dr. Dujardin Beaumetz, having studied the subject more closely by experiments on animals and by personal observations of the effects of strychnia in the human subject, has come to the conclusion that, whilst the drug is capable of controlling the functional manifestations of drunkenness, it had no influence for good over the effects of alcohol on the different organic tissues of the body. He would therefore recommend that strychnia be left in its proper place as a therapeutic agent of great value in certain forms of disease, to wit, those produced by the abuse of spirituous liquors, and should not be introduced for daily use under the pretence of imbibing the antidote with the poison.

The Prefect of Police has issued some strict regulations respecting lodging-houses or furnished apartments in Paris, as these have been found in a notoriously unsanitary condition. No room is to be let that did not measure two and a half meters in height, and did not have a cubic space of fourteen cubic meters per occupant. The walls and floors are to be regularly cleaned, and proper means of ventilation should be provided for each room. Cellars are not allowed to be used as dwellings. To carry out these regulations the lodging-houses will be periodically visited by a staff of inspectors, and any infringement of the above rules will be severely punished.

A. B.

MISCELLANEOUS.

NEW BOOKS.

Leube, Wilh. Ueber die Bedeutung der Chemie in der Medicin. 8vo. 56 pp. Berlin: Hirschwald.

Verhandlungen der deutschen Gesselschaft f. Chirurgie, 2. Congress Abgeh. zu Berlin vom 4–7 April, 1883. Berlin: Hirschwald.

Wagner, Adph. Ueber die Hernia properitonealis. Inaugural Dissertation 8vo. 118 pp. Dorpat: Schnakenburg.

Wernich. Lehrbuch f. Heildiener. Mit Berueecksicht der Wundenpflege. Krankenaufsicht u.. Desinfection. 8vo. viii. Berlin: Hirschwald.

Zabludowsky, J. Die Bedeutung der Massage f. die Chirurgie u. ihre physiologischen Grundlagen. 8vo. 39 pp. Berlin: Hirschwald.

Zeckendorf, Emil. Ueber die Pathogenese der Bauchtympanie, ueber Beiträgen zur Lehre vom Stoffwechsel bei der Hysterie. Inaugural dissertation. 8vo. 37 pp. Göttingen: Vandenhoeck & Ruprecht.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT UNITED STATES ARMY, FROM JANUARY 25, 1884, TO FEBRUARY 1, 1884.

Alexander, Charles T., Major and Surgeon: so much of Par. 7, S. O. 211, Sept. 14, 1883, as directs him to report in person to the Commanding General Department of the Missouri, for duty, is revoked, and he will, upon the expiration of his present leave of absence, proceed to St. Louis, Mo, and assume duty as attending surgeon and examiner of recruits in that city. (Par. I, S. O. 21, A. G. O., January 25, 1884.)

Elbrev, Frederick W., Captain and Assistant Surgeon: present leave of absence extended 6 months. (Par. 9, S. O. 24, A. G. O., January 25, 1884.)

LIST OF CHANGES IN THE MEDICAL CORPS OF THE NAVY, DURING THE WEEK ENDING FEBRUARY 2, 1884.

Passed Ass't Surgeon Robt. Whiting, detached from the Hospital, Norfolk, and to the "Colorado."

Ass't Surgeon H. B. Fitts, detached from the "Jamestown," and ordered to Coast Survey Str. Gedney.

Passed Ass't Surgeon F. Anderson, granted leave of absence for six months.

P. A. Surgeon S. W. Battle, detached from U. S. S. Gedney, and placed on sick leave.

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